The

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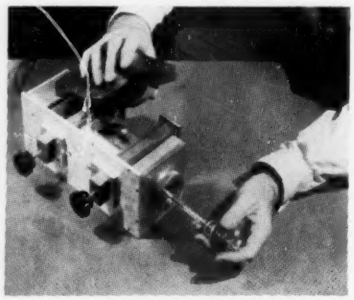
A GREAT AMPLIFIER TUBE IS PERFECTED FOR TELEPHONY

A new transcontinental microwave system capable of carrying four times as much information as any previous microwave system is under development at Bell Laboratories. A master key to this development is a new traveling-wave tube of large frequency bandwidth.

The traveling-wave amplifying principle was discovered in England by Dr. Rudolf Kompfner, who is now at Bell Laboratories: the fundamental theory was largely developed by Labs scientist Dr. John Pierce. Subsequently the tube has been utilized in various ways both here and abroad. At the Laboratories it has been perfected to meet the exacting performance standards of long distance telephony. And now for the first time a traveling-wave tube will go into large-scale production for use in our nation's telephone system.

The new amplifier's tremendous bandwidth greatly simplifies the practical problem of operating and maintaining microwave communications. For example, in the proposed transcontinental system, as many as 16 different one-way radio channels will be used to transmit a capacity load of more than 11,000 conversations or 12 television programs and 2500 conversations. Formerly it would have been necessary to tune several amplifier tubes to match each channel. In contrast, a single traveling-wave tube can supply all the amplification needed for a channel. Tubes can be interchanged with only very minor adjustments.

The new amplifier is another example of how Bell Laboratories research creates new devices and new systems for telephony.



Left: A traveling-wave tube. *Right:* Tube being placed in position between the permanent magnets which focus the electron beam. The tube supplies uniform and distortionless amplification of FM signals over a 500 Mc band. It will be used to deliver an output of five watts.

BELL TELEPHONE LABORATORIES

WORLD CENTER OF COMMUNICATIONS RESEARCH AND DEVELOPMENT

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Cover: Interior of the Human Heart

[*Courtesy of Moody Institute of Science, Los Angeles, California, see page 267*]

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Science and Technology

All inquiries concerning items listed here should be addressed to The Scientific Monthly, *Room 740, 11 W. 42 St., New York 36, N.Y. Include the name(s) of the manufacturer(s) and the department number(s).*

Electrocardiograph

In a light and compact electrocardiograph, transistors and other small components have been used. A three-stage, vacuum-tube input amplifier drives a three-stage transistor amplifier, which in turn drives the galvanometer recorder. The galvanometer is very much smaller than those previously used. Total weight is 18 lb. (Sanborn Co., Dept. M605)

Chromatograph

Vapor chromatograph is designed to take three rough cuts of a total sample, without preseparation, and make three simultaneous analyses of the cuts. Three stages operate in series, unresolved components being fed into the following stage. Separate recorders indicate separated bands. Series operation, or operation of each column separately, is optiona' one unit is designed to operate from ambient temperature to 300°C, a second from ambient to 225°C, and the third is modified for use with refrigerated baths. (Perkin-Elmer Corporation, Dept. M638)

Ultraviolet Microscope

Television techniques are used by an ultraviolet microscope to translate three selected ultraviolet wavelengths into three primary colors. Magnifications from specimen to screen of 4000 to 25,000 are possible. Resolution is of the order of 0.2 μ. A microspectrophotometer attachment displays the absorption of any selected ultraviolet wavelength along any of the horizontal scan lines. Absorption curves are displayed on a 5-in. screen. A quick-processing camera is provided for recording the curves. (Neutronics Research Co., Dept. M608)

Instrument Shelter

Shelter for use with tracking instruments consists of a steel cylinder, which is 10 ft in diameter and 4 ft high, topped by a hemispherical dome of reinforced plastic. The observation slot is 5 in. wide and extends 5 deg past the zenith. Rotation, which is not restricted in either direction, is accomplished by an electrohydraulic drive unit with sprocket and chain. Maximum rotational velocity is 40 deg/sec. Maximum error between tracking instrument position and dome position is 7 deg. (Coleman Engineering Co., Inc., Dept. M609)

Centrifuge

A new centrifuge furnishes a centrifugal field of 51,000*g*. Twenty accurately regulated speeds from 2500 to 25,000 rev/min may be selected by interchanging pulleys. Heat caused by windage friction is minimized by a combination of rotor insulation and ventilation. Convective stirring is said to be virtually eliminated. (Spinco Division, Beckman Instruments, Inc., Dept. M611)

Computing Device

Manually operated computing device performs harmonic analyses and curve fittings on plotted or recorded curves. A curve with maximum amplitude of 5.9 in. and horizontal span of 9 in. can be accommodated. A longer curve may be subdivided for analysis. (Gerber Scientific Instrument Co., Dept. M584)

Data Presentation Unit

Up to 20,000 alphanumeric characters per second can be presented by a new data presentation unit. The storage-type oscillograph tube used retains the display until it is intentionally erased. Any one of 64 characters, approximately 1/8 in. high, may be selected and recorded in any one of 1024 positions. A total of 400 characters may be displayed at one time. (Advanced Electronics Manufacturing Corp., Dept. M589)

Rocking Table

Control systems may be simulated by a rocking table. The unit has two axes that are controlled by servomechanisms. Angular rotation about each axis is controlled by electric signals supplied to the servo amplifiers. Motions about the two axes are independent. The table measures 16 by 10 in. and has a capacity of 30 lb. Maximum deflection is ± 15 deg, and maximum angular velocity is 120 deg/sec. Response is flat to ± 0.5 db up to a frequency of 1.5 cy/sec. (Short Brothers and Harland, Ltd., Dept. M596)

Planimeter

Planimeter for integrating linear strip-chart records is designed to accept charts up to 3 7/16 in. wide. The curve is followed by a pointer that is positioned by a hand-controlled knob. A variable-speed, foot-control drive permits the operator to adjust the speed of traverse to his ability to follow the curve. Optimum accuracy is ± 0.1 percent. A similar instrument is available for integrating square-root charts. (Librascope, Inc., Dept. M592)

Microscope

Remote-control microscope for nuclear metallography is based on designs of the United Kingdom Atomic Energy Research Establishment, Harwell. The inverted microscope is provided with tubular optical devices, called "transport optics," one for illumination and another for visual microscopy or photomicrography at a distance of 30 in. Several light sources are available. These are mounted outside the shielding. An objective changer, for six objectives and a micro hardness tester, is remotely controlled. The optical bench on the viewing side will accommodate a reflex camera, a cine camera, or a television camera. (Shandon Scientific Co., Ltd., Dept. M614)

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Measuring Geologic Time

ADOLPH KNOPF

Dr. Knopf received his training in geology at the University of California. He joined the U.S. Geological Survey in 1906 and, for the next 5 years, engaged in the study of the areal geology and ore deposits of Alaska. He then pursued similar investigations in Montana, California, Nevada, Colorado, and other western states. From 1920 to 1951 he was a member of the faculty of Yale University, being named Silliman professor of geology in 1937 and Sterling professor in 1938. He now holds the position of visiting professor of geology at the School of Mineral Sciences, Stanford University.

IN recent years our earth has been aging a billion years each decade. Between the beginning of the present century and 1930, an age of the earth of 100 million years had become generally accepted. In that year it was suggested that, in the light of the new discoveries of geology and radioactivity, the earth is at least 2000 million years old (*1*). Now, in 1957, we are envisaging an age of 4500 million years, and the end of the enormous lengthening of time appears to be in sight. Astronomers had estimated that the universe began to expand 1860 million years ago. However, this figure became geologically unacceptable when it became apparent that it was less than the age of the oldest rocks on our own planet. Recently, the distance of the Andromeda nebula was redetermined at Palomar and was found to be twice as great as it had previously been calculated to be. The distance of the Magellanic Cloud was also redetermined and set at twice the earlier figure. Since these distances were the yardsticks for measuring the extragalactic distances, all distances were doubled. These findings, together with Hubble's rate of recession of the galaxies, indicate that the age of the universe is 4000 million years—a figure which is in much better agreement with the age deduced from the rocks of our earth than previous estimates had been.

Our concept of geologic time has thus been increasing enormously, and this extension is a remarkable item in the history of ideas. At this point I may allude to the well-known estimate made by the Anglican Archbishop Usher, in 1654, that the earth was created in 4004 B.C. (Later, this estimate was improved upon and refined by the learned John Lightfoot, vice chancellor of Cambridge University, the greatest Hebrew scholar of his day. He declared that God had created Adam out of the dust of the earth on the morning of Friday, 17 September at 9 o'clock. I have this information from Shotwell's absorbing book on *The History of History*) (*2*). It was therefore something of a surprise to find Shakespeare's Rosalind saying, in *As You Like It*, which was produced in 1599, more than 50 years before the archbishop's pronouncement, "The poor world is almost 6000 years old, and in all this time there was not any man died in his own person, *videlicet*, in a love cause." Had Rosalind taken a modern elementary course in geology, she would have soon felt that she had made a grievous understatement!

How ingrained had become the belief that the earth was 6000 years old is shown by the first proposal ever made to measure the age of the earth quantitatively. In 1715 the Astronomer Royal, Edmund Halley, wrote "A short account of the

Cause of the Saltness of the Ocean; with a proposal, by help thereof, to discover the age of the world." He suggested that if the saltness of the ocean were measured at intervals of a few centuries, the rate of increase, and therefore the age of the ocean, could be determined. He lamented the ancient Greek and Latin authors had not handed down to us a record of the degree of saltness of the sea as it was some 2000 years ago, for, said he, "it can not be doubted but that the difference between what is now found and what then was, would become very sensible." Perhaps, he prophesied, the world would be found to be much older than many had hitherto imagined. The assumption that underlay Halley's proposal was that the ocean increases in saltness at a rate that is measurable in terms of human records.

Nearly 200 years elapsed, however, before a method was devised to measure the age of the ocean in years. In 1899 the brilliant Irish geologist Joly (*3*) estimated the age of the ocean, in years, as follows. The amount of sodium carried to the ocean each year by the rivers of the world is accurately known. If, then, we divide the amount of sodium in the ocean by the amount brought to the ocean annually, we have the age of the ocean—90 million years. There is a very seductive simplicity about this estimate. However, in making it, several assumptions had to be made. The greatest is that the rate at which the rivers have been wearing down the continents and bringing sodium in the form of salt to the ocean has been constant. As a matter of fact, the earth has recently passed through an epoch of widespread mountain-making, as a result of which the continents stand relatively high above sea level. The wearing away of the lands by erosion has therefore been speeded up, and the sodium that is thus released from the rocks is carried to the sea more abundantly than was the case during most of geologic time. How much faster is the present rate at which sodium is being delivered to the sea cannot be even roughly determined. However, because of the apparent logical rigor of this method, the figure of 90 or 100 million years for the age of the earth became generally accepted, and, in fact, long interfered with the favorable reception of far greater estimates based on new discoveries.

Chronology of Rocks of the Earth's Crust

The oldest method of measuring geologic time is by determining the thickness of the beds laid down during that time and multiplying the thickness by the rate at which these beds are supposed to have been deposited. In 1905, Sollas (*4*) estimated that 265,000 feet of strata had accumulated since the beginning of Huronian time, which was then thought to be near the beginning of geologic time. The figure of 265,000 feet was obtained by adding together the maximum thicknesses of the strata that were deposited during each of the successive geologic periods. Sollas was greatly impressed by Kelvin's estimate of 20 to 40 million years as the age of the earth. "Once more geology is put under bondage, not however as in her youth, tethered to a mere 6000 years, but free to roam through the ample magnitude of 30,000,000 years." By taking the rate of accumulation as 1 foot in a century, "as the evidence seems to indicate," Sollas concluded that more than 26 million years had elapsed during the time in which the 265,000 feet of strata were accumulating.

Since nowhere has 265,000 feet of strata been laid down in one place, geology, in building up such a "geologic column," is obliged to use the methods of stratigraphy and paleontology. The application of these methods to the geologic record has recently been presented by Stubblefield (*5*). From the beginning of the Cambrian onward, the sedimentary rocks contain fossils by means of which the sequence of the strata in time can be established. From this fact it follows that other means must be used in determining the age and succession of Precambrian rocks than those that are used for the Cambrian and younger rocks. For the Precambrian rocks, the methods of absolute age dating made possible by the numerous methods based on radioactivity have become essential; for the younger rocks, stratigraphy and paleontologic control have established a remarkable geochronology. It has been a chronology without years, however, and one of the chief purposes of this article is to show what progress in absolute dating has been made.

The major time units since the Precambrian eon are the eras Paleozoic, Mesozoic, and Cenozoic. The strata that represent these eras are divided into systems, beginning with the Cambrian system. The time during which the strata that comprise a system were formed is called a "period." Most of the systems are subdivided on a paleontologic basis into "Lower," "Middle," and "Upper." Thus, we have Lower, Middle, and Upper Cambrian and, in time phraseology, Early, Medial, and Late Cambrian; but the distinction between the strata and the time represented by them is more often honored in the breach than in the observance. It is, for example, widely customary to speak of "Lower Cambrian rocks" and "Lower Cambrian time."

The smallest unit of time is represented by a "zone." Formally, a zone is the smallest thickness of strata that is characterized by the presence of a distinctive flora or fauna. The same zone may

range in thickness, from place to place, from a few inches to hundreds of feet. The association together of fossils of several species is more essential to the defining of a zone than is the presence of one or two particular species, because any one species may have a range in time, from district to district, on account of migration or of differing environments. For convenience, each zone is named for a particular fossil (*6*). An ideal index fossil has four features: (i) It has a short vertical range (indicating that the lifetime of the species was short); (ii) it has a wide horizontal range; (iii) it is independent of lithic facies—that is, it may occur in sedimentary rocks of widely different composition; and (iv) it can be easily recognized (*7*, p. 12).

For construction of a single time scale of worldwide applicability, zones are not suitable, because they are generally too local in geographic extent and in vertical range. "For correlation over long distances, where zones are horizontally too restricted and vertically too precise, a larger stratigraphic unit is required. It must correspond to groups of zones and be capable of universal extension by means of overlapping correlations, although based ultimately upon a standard zonal succession at a type locality or in a type area" (*8*). This larger stratigraphic unit is called a stage.

These procedures have undoubtedly, so far, been most successfully applied to the Jurassic system (*7*). This system has been divided, in England, into ten stages, from Hettangian (J1, *9*) to Portlandian (J10), comprising 58 ammonite zones, and a final stage, the Purbeckian (J11), which, having been laid down in fresh water, contains no ammonites and has been subdivided therefore on the basis of ostracods into three zones (*7*, p. 19).

An urgent task for geology is to determine, in years, the length of the eras, periods, and "ages" (time spans of the stages) and, eventually of the zones. Not a single one of them—eras, periods, and ages, let alone zones—has yet been reliably determined. This statement is possibly surprising in view of the fact that almost any modern writer can produce a geologic timetable that gives precise datings and lengths of the eras and systems and even of some of the smaller subdivisions [Holmes (*10*); Kay (*11*); Schindewolf (*11*); Sonder (*11*)]. Sonder, in fact, gives the absolute lengths of the stages of the Permian, Triassic, Jurassic, Cretaceous, and Tertiary. These figures have been obtained in various remarkable ways. Ultimately, however, they are tied to three dates based on atomic disintegration: 60 million years, the age of the pitchblende at Central City, Colorado; 220 million years, the age of the pitchblende at St. Joachimsthal, Bohemia; and 440 million years, the age of the uranium-bearing shale at Gullhögen, Sweden. The age of the Swedish shale is the only one of these that is paleontologically controlled, by the occurrence in the shale of Late Cambrian trilobites, which are correlated with the middle Franconian of the North American time scale [Howell and Lochman, Westergård, Berg (*12*)]. The other two—Colorado and St. Joachimsthal—are less securely tied into the biochronologic scale.

All other absolute ages have been derived from the three radioactive tie points by interpolation based on thicknesses of strata or by "reasoned guesses," to use the phrase employed by Simpson (*13*) in explaining how he constructed his absolute time chart for the Tertiary. Holmes in 1947 (*10*) built two time scales, called "A" and "B," "based on maximum thicknesses and control points fixed by lead-ratios." The B scale is regarded by him as the more probable, but the geologic evidence appears to support more strongly parts of the A scale. There are three difficulties in building up such scales: (i) The boundaries between the systems are controversial—for example, between Devonian and Carboniferous, between Triassic and Jurassic, between Cretaceous and Paleocene; (ii) the control points, except for that of the Swedish scale, are not precisely located; and (iii) the thicknesses of strata are not reliable measures of time.

In 1905 Sollas (*4*) obtained 183,000 feet as the maximum thickness of strata accumulated since the beginning of Cambrian time; in 1931 Schuchert (*14*), in assembling the data for North America alone, got 259,000 feet and expressed the conviction that, when the world's maximum thicknesses have been compiled, these will total 400,000 feet. In 1947 Holmes (*10*) obtained 387,000 feet as a total. Kay (*15*), in 1955, presented data much more nearly complete than any that had been previously assembled, and these aggregate at least 398,000 feet—a figure almost identical with that predicted by Schuchert in 1931. Although the total given by Holmes and the total based on Kay's data are substantially alike, they are summations of items of considerably differing magnitudes; for example, the Silurian is credited by Kay with a maximum of 33,000 feet but with only 20,000 feet by Holmes; the Oligocene, with 26,000 feet by Kay and with 15,000 feet by Holmes; and the Miocene, with 14,000 feet by Kay and with 21,000 feet by Holmes. Kleinpell (*16*) gets 24,000 feet as a complete and unbroken sequence through the marine Miocene of California. When eventually a new summation of thicknesses is prepared—one that is based on the stages of the systems, paleontologically controlled—a much greater total than 400,000 feet will undoubtedly be obtained.

The great differences in the estimates of maximum thickness of many of the systems manifestly

indicate that thicknesses are unreliable measures of geologic time. As long ago as 1936 the conclusion had already been reached by Twenhofel (*17*) that estimates of time based on thicknesses of strata "are hardly worth the paper they are written on," and he presents detailed evidence in support of this revolutionary concept. Limitation of space prevents further marshaling of evidence here.

The nearly insuperable obstacle that one encounters in using thicknesses of rocks as measures of geologic time is the fact that the rocks generally give no internal evidence of the rate at which they were formed. Only a very few show a thin layering, or lamination, in which each lamina represents the sediment laid down in a year. Of the rocks that show such an annual lamination, those that have been studied most thoroughly are the Green River shales of Eocene age in Wyoming and Colorado. These annual layers—*varves* is their technical name—average less than 1/2000 foot in thickness, and since the Green River shales are 2600 feet thick, the time represented by their accumulation is about 6 million years. Green River time, which is possibly, but far from assuredly, one-third of the Eocene, is the longest span of time that has so far been measured by means of data obtained from the sedimentary history of the rocks themselves. This span of 6 million years is compatible with the great length of geologic time indicated by radioactive evidence, but there has as yet been no direct verification of the length of Green River time by radioactive methods. No one has yet measured the beginning and the end of Green River time by radioactive evidence, or even the beginning or end of the Eocene or of any other subdivision of geologic time. However, the methods of determining absolute ages have now become so numerous and are becoming so highly perfected that it will not be long before the lengths of the geologic time units will be accurately determined.

Helium Method

The helium method was the first of the methods based on atomic disintegration to be used to measure geologic time. Helium was early recognized to be a stable end-product of the radioactive transformation of uranium and thorium. Strutt (*18*) determined the amount of uranium contained in certain minerals (or the amount of thorium in thorianite) and the amount of helium held in the minerals, and, having measured the rate of production of helium from uranium and thorium, he was able to calculate the "ages" of the minerals. These pioneer datings of minerals of geologically known ages showed that the ages determined from the helium content fell into the proper geologic time sequence. Some of the Precambrian minerals gave astonishingly high "ages," far higher than was then considered to be probable. For example, Precambrian zircon and sphene gave "ages" of 600 and 700 million years. The great age that was thus indicated was soon realized to be a minimum, because much of the helium formed in the minerals had leaked away. Zircon was found to retain only about one-third of the helium generated from the uranium and thorium contained in it.

As the "lead method" of measuring geologic time grew in strength, the helium method, which gives only minimum values for the ages of minerals, fell into disuse. In 1928 Paneth devised a technique whereby quantities of helium as small as 1/1,000,000 cubic centimeter could be accurately measured. In the new helium method based on this technique, only minerals and rocks containing minute—one might say almost infinitesimally small—quantities of radioactive matter were selected. It was thought that the minute amount of helium generated in the mineral would be wholly retained in the mineral. Many rocks were examined by the new technique, and their ages, in years, were determined. Many of these ages appeared to be geologically acceptable.

In 1940, however, the new helium method collapsed. It was shown that if a rock is separated into its constituent minerals, there is marked difference in the ages given by the various minerals. For example, when the Palisade diabase was separated into its constituent minerals—plagioclase, pyroxene, and magnetite—the plagioclase gave an age of 36 million years, the pyroxene, of 103 million years, and the magnetite, of 134 million years. Manifestly the magnetite had retained more of the radiogenic helium formed within it than had the pyroxene and the plagioclase. When the magnetite was drastically purified by the removal of all adherent minerals (which are more radioactive than the magnetite), the indicated age of the magnetite was increased to 170 million years (*19*). Later work by Hurley (*20*) has cast doubt on the foregoing explanation, because determinations of age made before and after the minerals have been given an acid treatment suggest that all of them contain helium commensurate with their ages. A granite that gave by the helium ratio an age of 68 million years gave, after an acid wash, an age of 200 million years.

Because of such uncertainties about the helium age determinations, the method has again fallen into nearly complete disuse. One of the few recent determinations is that made by Gentner *et al.* (*21*) in 1954 on a potassium salt from Alsace, of early Oligocene age; they obtained an age "of only

10 million years," but after allowing for loss of helium by diffusion and the speeding up of this diffusion by the formerly higher temperature of the potash-salt bed, the indicated age increased to 25 million years. Because of the assumptions and corrections that are necessary, this figure, which appears to be low for early Oligocene, does not carry much conviction.

Lead Method

In 1905, Boltwood, of Yale University, suggested that lead is the ultimate product of the radioactive breakdown of uranium (*22*). This suggestion, sensational in its day, resulted from Boltwood's recognition that lead is invariably present in all uranium minerals. From the chemical analyses of 43 uranium minerals obtained from all parts of the world, Boltwood, in 1907, showed that the geologically older uranium minerals contain more lead than the younger minerals and that those of like geologic age have a like lead-uranium ratio. Boltwood then paid his debt to geology by giving us what has become known as the lead method of measuring absolute geologic time (*23*). In this pioneer attempt he ventured to compute the ages of ten minerals. These ages ranged from 410 million years for a uraninite from Connecticut to 2200 million years for another uranium-bearing mineral from Ceylon. These were stupendous figures, and they were not readily believed. Among those who soon accepted them, however, were Joly, Holmes, and Barrell.

In 1911 Holmes (*24*) began the great task of constructing an absolute geologic time scale. Boltwood had omitted to give the geologic ages of his analyzed radioactive minerals, and Holmes began to supply the deficiency. The amount of coordinated data was painfully small—that is, there were but few uraninites or other highly radioactive minerals whose geologic ages were accurately established and whose chemical composition had been accurately determined. As a matter of fact, this difficulty is still with us, for uraninites and other highly radioactive minerals almost invariably occur in pegmatite dykes and veins. Consequently, their geologic age cannot, in the nature of things, be accurately determined. At this time also (1913), Holmes wrote the first of his illuminating accounts on the age of the earth (*25*), which culminated, in 1956, in his paper "How old is the Earth?" (*26*). His answer is 4500 million years. The unreserved acceptance in 1917 by Barrell—in his classic paper on "Rhythms and the measurements of geologic time" (*27*)—of the new and immensely longer time estimates based on radioactivity helped to pave the way for the eventual acceptance of the longer time estimates.

Because of the manifest reluctance of geologists and others to accept the immense figures based on atomic disintegration, A. C. Lawson, chairman of the Division of Geology and Geography of the National Research Council, appointed, in 1923, a Committee on the Measurement of Geologic Time by Atomic Disintegration, "to see what it is all about." Under the able chairmanship of A. C. Lane, this committee, consisting of chemists, geologists, and physicists, actively stimulated research and promoted the fruitful cooperation between the investigators of the widely different disciplines that is necessary to solve the problems involved. Among its activities the committee published an annual report, in which the growth of the subject can be followed, and also, annually, a highly useful "Annotated bibliography of articles related to geologic time."

Almost at the moment that the committee was getting under way, F. W. Clarke, chief chemist of the U.S. Geological Survey and author of the famous *Data of Geochemistry*, announced, "It is now plain that the uranium-lead ratio is of very questionable value in determining the age of minerals" (*28*). This reluctance on the part of Clarke to accept the great ages that were indicated by the uranium-lead ratios was undoubtedly due to the fact that he had been engaged for several decades in improving and refining Joly's estimate of 90 million years as the age of the ocean and had reached the figure of 99,143,000 years, or in round numbers, 100 million years.

The lead method of determining absolute ages has, nevertheless, steadily grown in strength since it was first proposed by Boltwood. It has had some extraordinary and wholly unforeseen developments, but all of them have strengthened the method. In the first place, some years after Boltwood announced that lead is the stable end-product of the radioactive disintegration of uranium, thorium also was found to yield lead as a stable end-product, and this fact has to be taken into account. Furthermore, uranium was discovered to consist of two isotopes, both of them radioactive; one has an atomic weight of 238 and is therefore called "uranium-238" and the other is of atomic weight 235, the now famous uranium-235. Both are generating lead, but at greatly different rates; the U^{235} produces lead six times as fast as the U^{238}. Moreover, the atomic weights of the resultant leads differ. Uranium-238 produces a lead isotope of atomic weight 206, and U^{235} produces a lead isotope of atomic weight 207.

Most of the radioactive minerals used in determining ages contain not only U^{238} and U^{235} but also thorium, which, as I have just mentioned, is also producing lead. The thorium-derived lead

has an atomic weight of 208. Thus, when the chemist extracts the lead from such a radioactive mineral, the lead that he obtains—the so-called "radiogenic" lead—consists of a mixture of three isotopes of lead of atomic weights of 206, 207, and 208. It remained for the physicist to devise a means by which the proportions of these three leads can be determined. This was done by Aston, and the first mass spectrum of a radiogenic lead was obtained by him in 1929. It is now standard practice to have such a mass spectrum made in all reliable age determinations.

When amounts of uranium and thorium in a mineral have been accurately determined, we have four sets of data from which the age of the mineral can be calculated. These data are the following: Pb^{206}/U^{238}, Pb^{207}/U^{235}, Pb^{208}/Th^{232}, and Pb^{207}/Pb^{206}. When the four calculated ages agree, we can have full confidence in the indicated age.

The ratio between the two radiogenic leads Pb^{207} and Pb^{206} was regarded, by Nier (*29*), as being the most reliable index of age. Since the two leads have very nearly identical chemical properties, their proportionality is not likely to have been altered by any geologic vicissitudes, such as weathering, oxidation, hydration, and leaching, that might have affected the mineral in which they occur. An age determination based on this ratio is called the "lead-lead" method. An advantage of this method is the fact that neither uranium, thorium, nor lead needs to be determined and that all the work can be done in one laboratory. Recently the unprecedented number of 96 age determinations was made by this method at the University of Toronto. But for definitive results, experience shows that uranium, thorium, total lead, and the isotopic composition of the lead must be determined.

A recent example of age determination is afforded by the work done on uraninite from the Bob Ingersoll pegmatite, in the Black Hills, South Dakota (*30*). The analytical results in weight percentages are U, 64.55 ± 0.64 (average of two determinations by different methods); Th, 2.93 ± 0.04; and Pb, 17.01 ± 0.5. The ages are shown in Table 1.

Table 1. Ages of uraninite from the Black Hills, South Dakota (in millions of years).

Pb^{206}/U^{238}	Pb^{207}/U^{235}	Pb^{207}/Pb^{206}	Pb^{208}/Th^{232}
1580	1600	1630	1440

Three of the calculated ages agree, but that based on the Pb^{208}/Th^{232} ratio is discrepant; the reason for this is not known. Wetherill *et al.* conclude that "when the U^{238}-Pb^{206} and U^{235}-Pb^{207} ages agree for a fresh sample of uraninite, this age is probably the true age of the mineral."

The lead method was greatly strengthened when, in 1938, Nier estaplished the fact that all common lead—called also "ordinary lead" and "ore lead"—of whatever geologic age and provenance contains the isotope 204 along with the predominant isotopes 206, 207, and 208. Since the isotope 204 is not of radiogenic origin, its presence in the lead formed within a radioactive mineral indicates that the radiogenic lead is "contaminated"—in other words, that some common lead had become enclosed in the radioactive mineral at the time the mineral was formed. The contaminating lead would make the calculated age too great, and it must be allowed for. To make the proper correction, especially if the correction is a considerable one, an isotopic analysis of the common lead that had been deposited in the same district and at the same time as the radioactive mineral must be used. The necessity for this rigorous requirement has only been recognized within the past several years.

Nier also made the remarkable discovery that the relative abundances of the isotopes of common lead, regardless of geologic age and geographic source, differ considerably in spite of a nearly constant atomic weight of 207.21. In a broad way, the older the leads are, the smaller is the total proportion of Pb^{206}, Pb^{207}, and Pb^{208} relative to Pb^{204}. Manifestly, the common lead had been associated with uranium and thorium somewhere in the depths of the earth before it was deposited, some time later, as galena (PbS) in the place where we now find it. Thus, the common lead had become contaminated with radiogenic lead.

By extrapolating backward to the time when the amount of admixed radiogenic lead isotopes was zero, Holmes (*26*) has obtained the composition of the common lead when the substance of earth first became differentiated into crust, mantle, and core. That time was 4500 million years ago, and it can be regarded as marking the beginning of geologic time. Less hypothetical are the conclusions based on the isotopic composition of the common lead from the Rosetta Mine, Transvaal,

Table 2. Isotopic composition of lead from the Rosetta Mine, Transvaal, Union of South Africa.

Pb^{204}	Pb^{206}	Pb^{207}	Pb^{208}	Reference
1.00	12.65	14.27	32.78	Russell *et al.* (*31*)
1.00	12.58	14.11	32.77	Bate and Kulp (*55*)

Union of South Africa. Its isotopic composition is given in Table 2. From these data Russell *et al.* (*31*) computed the age of the Rosetta galena as being 2950 ± 70 million years; Holmes and Cahen (*32*), as 3380 million years.

Russell *et al.* think that galenas older than 1000 million years can well be dated by means of the isotopic composition of their leads, but Houtermans, Geiss, Ehrenberg, and others have dated many leads that are much younger, even as young as late Tertiary. Generally, the age thus calculated does not coincide with the geologic age of the deposit in which the galena occurs. The suggestion has therefore been made that this age (p) denotes the time at which the ore-forming solution separated from the magma and the time, consequently, after which it was not subjected further to change by addition of radiogenic lead. Consequently, p can agree with the geologic age of the ore only if the ore had been immediately deposited. If the ore was not formed immediately after its constituents had separated from the magma, then p (the "magmatic age" of the ore) is greater than the geologic age of the ore body in which it now occurs. This hypothesis can manifestly be improved geologically; at any rate, we can appreciate what a powerful tool the isotopic composition of common lead gives us in deciphering the origin of lead ore deposits.

A variant of the lead method, devised in 1952 by Larsen, Keevil, and Harrison (*33*) as a rapid means of determining the age of rocks, is known as the "Larsen" or "lead-alpha" method. Zircon is the mineral that is chiefly used, on the theory that, because the atomic radius of zirconium (0.82A) differs so much from that of lead (1.32A), the zircon would contain no primary lead which it might have acquired during the magmatic consolidation of the igneous rocks in which it occurs. A spectrograph is used to determine the amount of lead in the zircon, and alpha counters are used to determine the amount of helium given off per milligram of zircon per hour. The approximate age is given by

$$t = \frac{C\mathrm{Pb}}{\alpha}$$

where $C = 2480$, Pb equals lead in parts per million, and α equals number of alpha particles emitted per milligram, per hour. The results have proved to be uncertain, however. When the isotopic composition of the lead is ascertained, the four ages that are then calculable are, as a rule, highly discrepant. Zircon from the granite at Cape Town, Union of South Africa, gives the results shown in Table 3.

Table 3. Calculated ages of zircon from Cape Town, Union of South Africa (in millions of years).

Pb^{206}/U^{238}	Pb^{207}/U^{235}	Pb^{207}/Pb^{206}	Pb^{208}/Th^{232}
330	354	525	237

Nicolaysen (*34*) has recently made a careful study to determine the cause of these discrepancies. He concludes that "if these zircons crystallized 590 million years ago, and a constant diffusion coefficient has governed the loss of lead isotopes throughout the history of the mineral, then the present pattern of 'discrepant' lead-uranium and lead-lead ages would result."

The unreliability of the lead-alpha method, when applied to zircon may, in some cases, be due to the fact that the host rock (generally granite) may have been formed by the fusion of sedimentary rocks at the bottom of a down-folded geosyncline, or may have been modified by the melting of zirconiferous xenoliths. Some such explanation is indicated by the remarkable results obtained by Schuermann *et al.* (*35*) in processing 2000 kilograms of the Lausitz granodiorite of Germany. Zircon was found to occur in two distinct varieties, one of which gives a provisional age of 280 million years and the other, of 550 million years.

Rubidium-Strontium Method

Rubidium has long been known to be radioactive; it gives off beta rays and, consequently, it was known, from the theory of radioactive transformations, that rubidium-87 changes to strontium-87. In 1938 Strassmann and Walling (*36*) isolated the radiogenic strontium from a rubidium-bearing lithium mica (lepidolite) that they had obtained from southeastern Manitoba from a pegmatite known, by the lead method, to be about 2000 million years old. The strontium proved to be nearly 100 percent pure Sr^{87}, whereas ordinary strontium consists of four isotopes, of which Sr^{87} constitutes only 7.02 percent. The half-life of Rb^{87} was calculated to be 6.3×10^{10} years, and Hahn and Walling suggested that a new method was now available for dating rubidium-bearing minerals and rocks. They were optimistic about the potentiality of the strontium method, especially for determining the ages of ancient Precambrian rocks. More recently, as the result of the invention of refined techniques—the isotope dilution method, in particular—rocks as young as 60 million years have been measured. The strontium method has an advantage in that only a single transformation is involved in the change of Rb^{87} to Sr^{87}. Another

advantage is the fact that rubidium is widely distributed in potassium feldspars and micas, albeit in small amounts; this makes it possible to date many more rocks than is possible by the lead method.

Ahrens, beginning in 1946, was the first to employ the strontium method and, in the succeeding years, made a large number of age measurements by optical spectrographic methods (*37*). This investigation showed extremely great ages in the older portions of the earth's crust, especially in Southern Rhodesia and adjacent regions—ages of between 2000 and 3000 million years. In 1952, age determinations were first made (by investigators of the Department of Terrestrial Magnetism and the Geophysical Laboratory, both of the Carnegie Institution of Washington) by means of a new method in which stable isotope dilution and mass spectrometric techniques were used. Within a short time it became apparent that the rubidium-strontium method was giving much greater ages than those that were obtained by the lead method. The figure for the half-life of rubidium that was being used—as high as 6.42×10^{10} years—was found to be too great. If the rubidium-strontium ages of micas and microclines are calculated on the basis that the half-life of Rb^{87} is 5×10^{10} years, and if these are compared with the concordant uranium-lead ages obtained for uranium minerals that occur in the same pegmatites and that are therefore of the same age, excellent agreement is found, as is shown by Aldrich (*38*). Later in 1956, Huster and Rausch were reported to have determined, by direct counting experiments, that the half-life period of Rb^{87} is 4.9 to 5.0×10^{10} years.

A momentous advance in the use of the rubidium-strontium method was made in 1956 by Cormier *et al.* (*39*). Eight glauconites, from six different geologic horizons, were measured by a mass spectrometric isotope dilution method. The ages obtained range from 60 million years, for a Paleocene glauconite, to 470 million years for one of Lower Cambrian age. These ages have been computed on the basis of the newly accepted value for the half-life of Rb^{87} : $T^{1/2} = 5 \times 10^{10}$ years.

The Lower Cambrian glauconite was obtained from the *Olenellus*-bearing glauconite beds that constitute the top of the St. Piran sandstone on Mount Whyte, west of Lake Louise, Alberta, Canada (*40*). The great significance of the age measured—470 million years—is that it is the only reliable absolute age determination we as yet have that is close to the beginning of Cambrian time. Since it is but a single determination, however, it is of only provisional value. It strengthens, however, the belief that the Cambrian began approximately 500 million years ago.

Potassium-Argon Method

In 1905, potassium was discovered to be feebly radioactive; it was found to emit beta rays. Later, in 1928, it was found to give off gamma rays as well. Not until 1937 was it discovered that all the radioactivity of potassium results from the decay of the isotope potassium-40, which constitutes but a minute fraction of the element potassium—approximately 1/8400. The K^{40} undergoes a dual transformation—one part decays to calcium-40 and one part to argon-40. How much changes to Ca^{40} and how much to A^{40} (determined by the branching ratio λ_e/λ_β) has been difficult to measure accurately. The latest figure for this ratio (*41*) is 0.1235. The half-life of K^{40} is 1310 million years.

The conclusion of Weizsäcker in 1937, from theoretical considerations, that A^{40} is one of the products of the radioactive disintegration of potassium was verified by Aldrich and Nier (*42*) in 1948. They showed that four potassium-bearing minerals, ranging in age from 200 million years to 1600 million years, contained radiogenic argon in appropriate amounts, and they then suggested that a new method for determining the ages of rocks was possible. The technique for measuring the amount of argon that is radioactively produced in minerals has since been greatly refined, and thus the potassium-argon method of dating rocks was born.

The potassium-argon method of dating minerals has several great advantages. One is the abundance and wide distribution of potassium minerals in the earth's crust—namely, potassium feldspar and biotite. The second, and enormously important, advantage is the fact that the geologic ages of many of the rocks that contain the potassium minerals can be accurately determined. If the rock that contains the potassium minerals also contains fossils or is associated with fossiliferous rocks, its geologic age is paleontologically controlled.

A wholly unexpected discovery, made during the development of the potassium-argon method, was the fact that the potassium feldspars—orthoclase and microcline ($KAlSi_3O_8$)—retain only about 75 percent of the argon that is generated within them, whereas biotite and other micas, despite their perfect cleavage, retain all or nearly all the argon formed within them. As a result of this discovery, investigators have turned, since early in 1956, from using feldspar to using biotite in determining the ages of igneous rocks.

A potassium-bearing mineral that is proving to be highly useful in dating sedimentary rocks is glauconite, $K(Fe^{+++},Al)(Mg,Fe^{++})Si_4O_{10}(OH)_2$. This is a mineral that forms in a marine environment; it is an "authigenic" product, formed con-

temporaneously with sedimentation. It is also proving to be highly useful in connection with the rubidium-strontium method described earlier.

Numerous potassium-argon age datings have already been made in several laboratories in the United States, Canada, and Germany. The oldest rock so far dated is a cobble in the basal conglomerate of the Bulawayan system of Southern Rhodesia (*43*). The age (calculated by using $\lambda = 0.55 \times 10^{-9}$ yr^{-1} and $R = 0.085$, where "$R = 0.085$" is an empirical calibration constant that corrects for loss of argon) is 3310 million years. The basal beds of the Bulawayan system consist of thick conglomerate, composed mainly of granite boulders. The basal beds rest with conspicuous unconformity on talc schists, intruded by granite like that of the granite boulders in the conglomerate. Accordingly, the Sebakwian system, which underlies the Bulawayan system, can tentatively be considered to be more than 3300 million years old. This age determination indicates that the Sebakwian rocks are the oldest rocks of the earth so far dated. The next essential step will be confirmation, by direct determination, of the age of the Sebakwian by more than one method—presumably by the potassium-argon and the rubidium-strontium methods—and by the use of several different minerals.

The potassium-argon method has been used to determine the age of the Forest City, Iowa, meteorite—a bronzite chondrite. Wasserburg and Hayden (*44*), using 0.085 as the value of the branching ratio *R*, found its age to be 4670 million years. The value 0.085, as was mentioned in the preceding paragraph, had been obtained as a calibration constant in measuring the ages of feldspars; consequently, it is uncertain or unlikely that this value holds for bronzite and for other minerals for which it has been used. Folinsbee *et al.* (*45*) determined the age of the Forest City meteorite as being 4240 million years by using the potassium-argon method, but they took *R* to be 0.11. According to Patterson (*46*) the age of the meteorite by the lead-lead method ("the most accurate method") is 4550 million years.

The potassium-argon method has recently been developed to such an extreme sensitivity that rocks that are less than 2 million years old have been successfully dated (*47*). The rhyolite at Sutter Buttes, California, was determined, on the basis of the argon-potassium ratio of its biotite, to be 1.57 million years old, and the geologically slightly younger andesite was found to be 1.69 million years old. Both absolute ages are virtually the same, being within the limits of experimental error. Both the rhyolite and andesite are known, from field evidence, to be Pliocene or Pleistocene.

The potassium-argon method thus gives promise of attaining a resolving power nearly as great as that of biochronology. In favorable circumstances—as, for example, the Jurassic system—the resolving power of biochronology is so great that it can distinguish no less than 58 world-wide ammonite zones, each of which is thought to represent a time-span of approximately 500,000 years (*48*). The attainment of a correspondingly high resolving power by the potassium-argon method will be a great event in the history of geochronology.

Carbon-14 and Other Methods

The carbon-14 method of age determination devised by W. F. Libby in 1947 is of great importance in dating the past 50,000 years (*49*). Neutrons produced by cosmic radiation react with atmospheric nitrogen at high altitudes to form radiocarbon (C^{14}), which then combines with oxygen to form carbon dioxide. Plants utilize the radioactive carbon dioxide along with the normal carbon dioxide; hence all living matter eventually contains radioactive carbon. The half-life of C^{14} is 5570 years. Some ten laboratories, scattered throughout the world, are determining ages by the radiocarbon method. Most of them are equipped to determine ages up to about 38,000 years; the extreme sensitivity of 53,000 years is reached by the laboratory at Groningen, Netherlands. In 53,000 years the radiocarbon has decreased to only about $\frac{1}{8}$ of 1 percent of the minute amount that was originally present.

Unlike the other radioactive methods of age dating, the radiocarbon method has not lengthened the previous estimates of geologic time but has cut down to one-half the long-accepted estimate of the length of postglacial time, giving a date of 11,000 years ago as the beginning of the final retreat of the Wisconsin ice sheet. The radiocarbon method is useful only for dating Recent and late Pleistocene time—the last few moments of geologic time.

Many other methods based on atomic disintegration have been proposed or are being developed, but it would take too much space to describe them here. One that is particularly desirable, since it would bridge the gap between argon age datings and the carbon-14 datings, has recently been outlined by Arnold (*50*). Beryllium-10 has been found to be a product of cosmic-ray bombardment in the atmosphere; it is a beta-ray emitter and has a half-life of 2.5 million years. This half-life period is long enough, if a method for using Be^{10} for radioactive age determinations can be developed, to date events in the Pleistocene and late Pliocene. The Pleistocene is conventionally considered to be 1 million years long, but this figure has not yet been confirmed by any objective evidence.

Absolute Ages of Geologically Dated Minerals and Rocks

Before 1956, only one absolute age determination had been made on paleontologically controlled material. That material was the Peltura zone of the remarkable marine oil shale in Sweden, which contains the uranium-bearing nodules known as "kolm"; it carries trilobites and other fossils, from which it is determined to be of very late Cambrian age. The kolm contains 0.462 percent uranium, which appears to have been precipitated out of the sea water and incorporated into the kolm at the time the kolm was forming. The isotopic composition of the radiogenic lead in the kolm was determined by Nier, in 1939, and yielded the very disconcerting result that the age, based on Pb^{206}/U^{238}, is 380 million years, whereas that based on Pb^{207}/Pb^{206} is 770 million years. Now Nier, it must be recalled, regarded the figure given by the Pb^{207}/Pb^{206} ratio as being the least subject to error and hence the most reliable. For the kolm, however, the figure 770 million years was clearly too large. No answer to this paradox was forthcoming until Wickman (*51*) proposed a solution. During the transformation of uranium to lead, one of the intermediate radioactive products is radon, a gas of half-life period of 3.82 days. Consequently, the possibility exists that some of the radon may escape. If some does escape, the amount of radiogenic lead ultimately formed will be too small. Therefore, the age given by the ratio Pb^{207}/Pb^{206} will be too large, and the age given by the ratio Pb^{206}/U^{238} will be too small. By solving two simultaneous equations involving these quantities, the probable real age is found to be 440 million years.

Three other absolute age datings have been fundamentally important in building up the absolute geologic time scale, but they are less securely placed in the geologic time column than is the Swedish kolm. One is the previously mentioned pitchblende from Central City, in the Front Range of Colorado. The mean of two closely concordant results obtained by Nier *et al.* (*52*), in 1941, gives an age of 58 million years or, in round numbers, 60 million years. This figure of 60 million years has long been used, especially by Holmes and Stille, to date the Laramide revolution and hence the beginning of Tertiary time. However, the Laramide orogeny is now known to comprise eight or more phases. These phases extended in time from late Cretaceous to the end of the Oligocene. The problem as now seen is: Which phase of the Laramide orogeny is dated by the pitchblende of Central City? According to T. S. Lovering, who has long studied the geology of Colorado in the field, the veins in which the pitchblende occurs are related as aftereffects of the intrusion of a porphyry stock that cuts through a great thrust, 50 miles long, known as the Williams Range thrust. This thrust has affected strata as young as Fort Union, of Paleocene age. The pitchblende is therefore post-Fort Union and is regarded as having been deposited at or near the end of Paleocene time (*53*).

The pitchblende from St. Joachimstal, Bohemia, constitutes another important tie point. Nier (*29*) in 1939, using a pitchblende that contained 42.3 percent of ordinary lead and 57.7 percent of radiogenic lead, obtained the figure of 227 million years as its age; since 1939, a slightly different value of the half-life of U^{238} has been adopted; this brings the Pb^{206}/U^{238} age to 223 million years or, in round numbers, 220 million years. The various German authorities—Stille, von Bubnoff, and Weyl—regard the pitchblende as being of Saalian age—that is, of latest Early Permian ("Unter Rothliegend") age.

The other valuable age-dating by the lead method, isotopically controlled, is based on a thorite from a pegmatite near Oslo, Norway. The calculated age, based on the Pb^{208}/Th^{232} ratio, is 224 million years; based on the Pb^{206}/U^{238} ratio, it is 233 million years—in round numbers, 230 million years. From the geologic evidence, the thorite is inferred to have been formed about the end of Early Permian time.

From lead-uranium ratios, the end of the Ordovician is known to be, roughly, 350 million years ago, and the end of the Silurian, about 300 million years ago.

In 1956, 19 or more absolute age determinations on geologically dated material became suddenly available. These 19, as well as that of the kolm of Sweden, are shown in Table 4. They are listed according to their order in the geologic time scale, beginning with the oldest, of late Early Cambrian age (470 million years) and ending with the Miocene (M4, the fourth of the six subdivisions of the Miocene). The corresponding absolute ages fall roughly into the proper sequence. The discrepancies point up the fact that the methods for determining absolute ages do not yet equal the resolving power of the biochronologic methods.

The Miocene (M4) 21 million years, according to a potassium-argon determination made on glauconite (*54*), is out of line with the ages determined for the Oligocene.

Particularly interesting are the two determinations of age made on the Hornerstown marl. An argon determination on glauconite by Wasserburg *et al.* (*43*) gives 50 million years, and a Rb^{87}/Sr^{87}

Table 4. Age determinations of geologically dated minerals.

Geological Age	Age (millions of years)	Locality	Mineral	Method
Miocene (M4)	21	New Zealand	Glauconite	Argon
Oligocene (O5)	16	New Zealand	Glauconite	Argon
Oligocene (O3)	20	New Zealand	Glauconite	Argon
Oligocene (O1)	25	Alsace	Sylvite	Argon and helium
Eocene (E5)	36 to 39	New Zealand	Glauconite	Argon
Paleocene	50	Hornerstown marl, N.J.	Glauconite	Argon
Paleocene	60	Hornerstown marl, Clayton, N.J.	Glauconite	Strontium
Paleocene	47	New Zealand	Glauconite	Argon
Cretaceous				
"Late" Cretaceous	62	Clayton, N.J.	Glauconite	Strontium
Maastrichtian (K12)	70	Navesink formation, Clayton, N.J.	Glauconite	Strontium
Campanian (K11)	69	Marshalltown formation, N.J.	Glauconite	Argon
Cenomanian (K7)	90	Crowsnest volcanics, Alberta	Feldspar	Argon
Albian (K6)	138	MacMurray, Canada	Glauconite	Argon
Late Middle Devonian				
Givetian	270	Elk Point formation, Saskatchewan	Sylvite	Argon
Late Middle Ordovician	380	Dubuque formation, Minn.	Orthoclase	Argon
Early Ordovician	375 to 381	Stenbrottet, Sweden	Glauconite	Strontium
Middle Upper Cambrian	440	Gullhögen, Sweden	Kolm	Lead
Upper Cambrian				
Franconian	440	Sparta, Wis.	Glauconite	Argon
Early Upper Cambrian	401 to 413	Central Texas	Glauconite	Strontium
Late Lower Cambrian	470	St. Piran sandstone, Alberta	Glauconite	Strontium

age determination on glauconite by Cormier *et al.* (*39*) gives 60 million years. Since the Hornerstown marl is said to be an almost pure bed of glauconite, from 5 to 30 feet thick, and to represent the whole of the Paleocene, future determinations of absolute age of the Hornerstown marl should be made on carefully selected material of accurately known stratigraphic position.

The absolute age of the Albian, the sixth of the 12 or 13 stages that make up the Cretaceous system, is given as 138 million years (*54*), but this is obviously a misfit.

Particularly interesting is a comparison of the absolute age of the kolm of Sweden and the recently determined absolute age of the Franconian glauconite of Sparta, Wisconsin. The paleontologic evidence indicates that the kolm and the glauconite are of the same, or of nearly the same, age —approximately middle Late Cambrian. The kolm, as was previously shown, is 440 million years old; the age of the glauconite, as it was determined by means of the potassium-argon method by Wasserburg *et al.* (*43*), is 440 million years. Paleontologic dating and absolute ages thus agree extraordinarily closely. The age given here—440 million years—has been recalculated from the authors' data by means of the decay constants that are used in calculating the ages of the other glauconites listed in Table 4[1].

Finally, the absolute age dating of the glauconite from the *Olenellus*-bearing beds that make up the topmost portion of the St. Piran sandstone of Alberta is extraordinarily important, as was mentioned earlier. This absolute age of 470 million years, determined on rocks that contain fossils of known paleontologic age, is so far the nearest that we have to the dawn of the Cambrian period, which marks the beginning of the Paleozoic era, when the oceans began to team with living organisms of all the phyla except the vertebrates.

Summary

The new evidence tends to strengthen the estimates that the Cenozoic era is approximately 70 million years long, the Mesozoic, approximately 130 million years, and the Paleozoic, 300 million years. Before the beginning of the Paleozoic era

there was a vast stretch of time, possibly 4000 million years long. Eight-ninths of geologic time had already passed before there began that portion of the earth's history which is generally held to be the most significant (*56*).

References and Notes

1. A. Knopf, in *The Age of the Earth*, Natl. Research Council (U.S.) Bull. No. 80 (1931), p. 8.
2. J. T. Shotwell, *The History of History* (Columbia Univ. Press, New York, 1939).
3. J. Joly, *Sci. Trans. Roy. Dublin Soc.* **7**, No. 2, (1899).
4. W. J. Sollas, *The Age of the Earth and Other Geological Studies* (Dutton, New York, 1905).
5. C. J. Stubblefield, *Advancement of Sci.* **11**, 149 (1954).
6. L. J. Wills, *The Physiographical Evolution of Britain* (Arnold, London, 1929), p. 62.
7. W. J. Arkell, *Jurassic Geology of the World* (Oliver and Boyd, Edinburgh, 1956).
8. ———, *Am. J. Sci.* **254**, 460 (1956).
9. The symbol Jl is used as a mnemonic form to indicate that the Hettangian is the first stage of the Jurassic system.
10. A. Holmes, "The construction of a geological time scale," *Trans. Geol. Soc. Glasgow* **21**, 145 (1947).
11. M. Kay, *North American Geosynclines*, Geol. Soc. Amer. Mem. No. 48 (1951), p. 93; O. H. Schindewolf, *Der Zeitfaktor in Geologie und Paleontologie* (Schweitzerbart'sche, Stuttgart, Germany, 1950), p. 22; R. A. Sonder, *Mechanik der Erde* (Schweitzerbart'sche, Stuttgart, Germany, 1956), p. 64.
12. B. F. Howell and C. Lochman, "Succession of Late Cambrian faunas in the Northern Hemisphere," *J. Paleontol.* **13**, 115 (1939); A. H. Westergård, "Supplementary notes on the Upper Cambrian trilobites of Sweden," *Sveriges Geol. Undersökn. Årsbok* **41** (1947); R. Berg, "Franconian formation of Minnesota and Wisconsin," *Bull. Geol. Soc. Amer.* **65**, 867 (1954).
13. G. G. Simpson, "A continental Tertiary chart," *J. Paleontol.* **21**, 481 (1947).
14. C. Schuchert, "Geochronology, or the age of the earth on the basis of sediments and life," in *The Age of the Earth*, Natl. Research Council (U.S.) Bull. 80 (1931), p. 10.
15. M. Kay, "Sediments and subsidence through time," in *The Crust of the Earth*, Geol. Soc. Amer. Spec. Paper No. 62 (1955), p. 672.
16. R. M. Kleinpell, *Miocene Stratigraphy of California* (Am. Assoc. Petroleum Geol., Tulsa, Okla., 1938).
17. W. H. Twenhofel, "Marine unconformities, marine conglomerates, and thicknesses of strata," *Bull. Am. Assoc. Petroleum Geol.* **20**, 701 (1936).
18. R. J. Strutt, *Proc. Roy. Soc. London* **A83**, 298 (1910).
19. P. M. Hurley and C. Goodman, *Bull. Geol. Soc. Amer.* **54**, 305 (1943).
20. P. M. Hurley, *ibid.* **61**, 1 (1950).
21. W. Gentner, K. Goebel, R. Präg, *Geochim. et Cosmochim. Acta* **5**, 124 (1954).
22. B. B. Boltwood, *Am. J. Sci.* **20**, 253 (1905).
23. ———, *ibid.* **23**, 77 (1907).
24. A. Holmes, *Proc. Roy. Soc. London* **A85**, 248 (1911).
25. ———, *The Age of the Earth* (Harper, New York, 1913).
26. ———, *Trans. Edinburgh Geol. Soc.* **16**, 313 (1956).
27. J. Barrell, *Bull. Geol. Soc. Amer.* **28**, 745 (1917).
28. F. W. Clarke, *The Data of Geochemistry*, U.S. Geol. Survey Bull. 770 (ed. 5, 1924), p. 322.
29. A. O. Nier, *Phys. Rev.* **55**, 153 (1939).
30. G. W. Wetherill *et al.*, *Geochim. et Cosmochim. Acta* **9**, 292 (1956).
31. R. D. Russell *et al.*, *Trans. Am. Geophys. Union* **35**, 301 (1954).
32. A. Holmes and L. Cahen, "African geochronology," *Colonial Geol. Surveys (London)* **5**, 32 (1955).
33. E. S. Larsen, N. B. Keevil, H. C. Harrison, *Bull. Geol. Soc. Amer.* **63**, 1045 (1952).
34. L. U. Nicolaysen, *Geochim. et Cosmochim. Acta* **11**, 41 (1957).
35. H. M. E. Schuermann *et al.*, *Geol. en Mijnbouw* **18**, 312 (1956).
36. F. Strassmann and E. Walling, *Ber. deut. chem. Ges. Jahrg.* **71**, 1 (1938).
37. L. H. Ahrens, "Radioactive methods for determining geological age," in *Physics and Chemistry of the Earth* (McGraw-Hill, New York, 1956), vol. 1, p. 44.
38. L. T. Aldrich, *Science* **123**, 874 (1956).
39. R. F. Cormier *et al.*, *Bull. Geol. Soc. Amer.* **67**, 1681 (1956).
40. F. Rasetti, *Middle Cambrian Stratigraphy and Faunas of the Canadian Rocky Mountains*, Smithsonian Inst. Publs. Misc. Collections **116**, No. 5 (1951).
41. G. W. Wetherill, "Radioactivity of potassium and geologic time," *Science* **126**, 545 (1957).
42. L. T. Aldrich and A. O. Nier, *Phys. Rev.* **74**, 876 (1948).
43. G. W. Wasserburg, R. J. Hayden, K. J. Jensen, *Geochim. et Cosmochim. Acta* **10**, 159 (1956).
44. G. W. Wasserburg and R. J. Hayden, *ibid.* **7**, 51 (1955).
45. R. E. Folinsbee, J. Lipson, J. H. Reynolds, *ibid.* **10**, 61 (1956).
46. C. Patterson, *ibid.* **10**, 230 (1956).
47. G. H. Curtis *et al.*, *Nature* **176**, 1360 (1956).
48. J. A. Jeletzky, *Bull. Am. Assoc. Petroleum Geol.* **40**, 693 (1956).
49. W. F. Libby, E. C. Anderson, J. R. Arnold, *Science* **109**, 227 (1949); W. F. Libby, *Am. Scientist* **44**, 98 (1956).
50. J. R. Arnold, *Science* **124**, 584 (1956).
51. F. E. Wickman, *Geol. Fören. i Stockholm Förh.* **64**, 465 (1942).
52. A. O. Nier, R. W. Thompson, B. F. Murphey, *Phys. Rev.* **60**, 113 (1941).
53. A. Knopf, "The geologic records of time," in *Time and Its Mysteries* (New York Univ. Press, New York, 1949), ser. III, p. 33; "Time in Earth history," in *Genetics, Paleontology, and Evolution* (Princeton Univ. Press, Princeton, N.J., 1949), p. 1.
54. J. E. Lipson, *Geochim. et Cosmochim. Acta* **10**, 149 (1956).
55. G. L. Bate and J. L. Kulp, *Science* **122**, 970 (1955).
56. Following is a list of selected comprehensive accounts of measurement of geologic time: L. H. Ahrens, "Radioactive methods for determining geological age," in *Physics and Chemistry of the Earth* (McGraw-Hill, New York, 1956), vol. 1, pp. 44–67; L. T. Aldrich, "Measurement of radioactive ages of rocks," *Science* **123**, 871 (1956); H. Faul, Ed., *Nuclear Geology, a Symposium on Nuclear Phenomena in the Earth Sciences* (Wiley, New York, 1954); A. Holmes, *The Age of the Earth* (Nelson, London, 1937); A. Knopf, Ed., *The Age of the Earth*, Natl. Research Council (U.S.) Bull. 80 (1931); J. L. Kulp *et al.*, "Present status of the lead method of age determination," *Am. J. Sci.* **252**, 345 (1954); J. L. Kulp, "Isotopic dating and the geologic time scale," in *The Crust of the Earth*, Geol. Soc. Amer. Spec. Paper 62 (1955), pp. 609–630; J. P. Marble *et al.*, "Symposium on the measurement of geologic time," *Trans. Am. Geophys. Union* **33**, 149 (1952); K. Rankama, *Isotope Geology* (McGraw-Hill, New York, 1954); F. E. Zeuner, *Dating the Past: an Introduction to Geochronology* (Methuen, London, rev. ed. 3, 1952).

Aspects of Insect Flight

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THE flight of insects can conveniently be discussed from four aspects: First, the paleontological aspect, in dealing with which we may speculate fearlessly regarding the origin and evolution of flight in insects; second, the 19th century, during which man first took the study of insect flight seriously and at the end of which he learned to fly himself; third, the last 50 years, in which improved techniques have enabled us to comprehend at least how an insect accomplishes straight, level flight; finally, we may look briefly into the future.

The first three aspects can be dealt with in increasing detail; for the last, we must return to speculation.

Paleontological History

First then, let us go back about a billion years and take a brief, broad look at the way things may have started. We shall take a somewhat narrow view of flight and might define it as controlled progress through the air.

A billion years ago life in some form had undoubtedly begun, but more than half of this period had to pass before the first insects put in their appearance. They had no wings. Plants of prostrate form there were, and insects undoubtedly fed on them and on their remains. Figure 1 shows the important events roughly in perspective. A hundred million years after the first insects, there were tall plants, growing perhaps 4 or 5 feet into the air, probably most of them emergent out of the shallow water of swamps. Almost immediately, geologically speaking, the first winged insects appeared. It is logical to suggest that insects first climbed up the plants, then glided, and finally flew from these elevated take-off sites, perhaps thereby escaping the attentions of predatory animals—maybe other insects or arthropods—hard on their heels.

There are two principal theories regarding the origins of wings themselves. One of them, the tracheal gill theory of Gegenbauer (*1*), supposes that insects passed directly from an aquatic existence to an aerial one and used as organs of flight the musculated, flaplike respiratory structures, similar to those seen in the aquatic nymphs of present-day May flies. Although there are many attractive features to this theory it now receives little support, largely because of other evidence regarding the origins of the spiracular respiratory system—an adaptation to terrestrial life. Clearly, however, since much of the vegetation at this time was emergent, a passage from the aquatic to the aerial environment by way of vegetation would have been possible. The alternative, the paranotal theory of Müller (*2*), cannot be regarded as proved, although there is no directly contradictory evidence. The best support comes from such fossils as those of *Stenodictya lobata* Brongniart, from the Upper Carboniferous, shown in Fig. 2. In this species the paranotal lobes, which are nothing more than lateral extensions of the dorsal plates or nota, can be seen on the first segment of the thorax and on the abdominal segments. The last two segments of the thorax carry the wings, which are presumed to be serially homologous with these structures. It should be noted, however, that fossils have yet to be found which show either functional wings on any other segment or simple paranota on the wing-bearing segments, and recent Soviet work (*3*) has revealed a variety of radically different vein arrangements in paranota of the prothorax of fossil insects, none of which appear comparable with venation in wings. Whatever theory is correct, it seems certain that the insects are the only group of animals which have acquired wings and mastered the air without sacrificing a pair of limbs or true appendages for this purpose.

So much for the origin of wings; for the origin of the flappings of these wings, we have much less to guide us. Forbes (*4*) has suggested that initially a tonic contraction of the basalar muscle—a probable step in the mechanism of bringing a gliding

insect in for a landing—was transformed into a clonic contraction. This may have happened as a result of countermanding instructions from the central nervous system. There is much evidence, however, that the muscle-thorax-wing system vibrates with its own natural mechanical frequency. Indeed, any very different frequency would make an impossible demand for power from the muscular system. This suggests that the initial flapping of wings was a simple mechanical vibration imposed on them by the stream of air rushing past them when the insect was gliding. The physical requirements for this to happen are very simple; one may in fact use the front wing of a grasshopper or a cockroach—among the most primitive wings in present-day insects—as a reed in a wind instrument. The note produced when air is blown past an insect-wing "reed" roughly matches that made by the flying insect. Once vibration had started in this way, a few changes in the distribution of sclerite and membrane and in the relative development of muscles would have permitted it to be maintained by muscular action. How easily these muscular changes are brought about is evinced by the numerous recent reports of autolysis and even redevelopment of insect flight muscles (*5*).

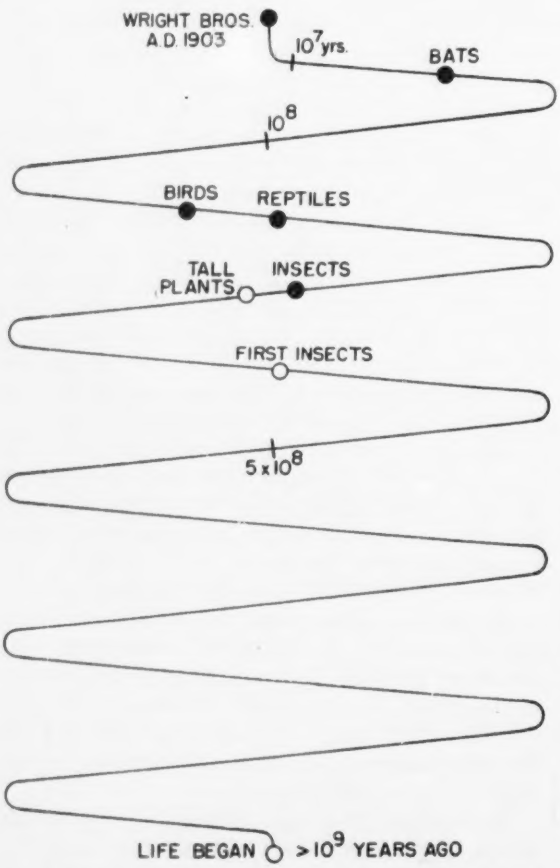

Fig. 1. Time chart of the evolution of flight. Solid circles indicate first flights, empty circles indicate other significant events.

Of particular interest in connection with the evolution of insects is the fact, brought out in Fig. 1, that insects had the air to themselves for about 100 million years before the first vertebrates followed them there—flying reptiles, perhaps forced to follow in self-defense. (Their experiments in this direction were probably helped by the abundant insect food they found there.) During these 100 million years of immunity, an explosive evolution and multiplication of the insects took place, to be followed undoubtedly by a catastrophic decimation as other groups of reptiles, birds, and finally bats followed the first vertebrate conquest of the air. The selection pressure of these several groups of predators, while decreasing the number of insects, must have forced an increase in the number of species. Forms adapted to avoid, evade, or delude these newcomers in the air were selected for survival. It is probably to this phase of the history of life that we owe the abundance of insect species in the world today, exceeding in numbers those of all other animals together.

Man learned to fly yesterday, geologically speaking. In doing so, as we shall see in a later section, he received inspiration if not instruction from the insects; it seems ungrateful of him to direct his new powers toward insect extermination.

The 19th Century

Aristotle, Leonardo da Vinci, and Borelli (*6*) stand out as early contributors to our knowledge of flight, but it was not until well into the 19th century that progress became at all rapid. Advances in the physical sciences at that time, and in particular in the art and science of photography, stimulated progress in investigations into flight. Indeed, the movements of most insect wings are so rapid that they defied elucidation until four tools were available: electronic flash illumination, instantaneous photographic exposures, the stroboscope, and finally motion pictures.

Wheatstone (*7*) used electronic flash in 1834, before the invention of photography, to observe rapid motion; Fox Talbot made a great impression in 1851 by using it to photograph a page of the London *Times* that was fixed to a spinning wheel. Its application to the study of insect flight was not reported until 1904, and it was more than a century after its first use before Germeshausen and Edgerton obtained their striking results.

The first "instantaneous" photograph, with an exposure of between 0.5 and 0.1 second, was taken by Gaudin in 1841, but much shorter exposures were necessary for insects. Marey was able to take series of instantaneous photographs of many animals by 1874.

The stroboscope was invented in Brussels by the Belgian physicist Joseph Plateau and independently, in Vienna, by Simon Stampfer in 1832. This invaluable device uses perodic flashes or other changes in light intensity to study high-frequency periodic movements of any kind. Plateau's son Felix made important contributions to insect physiology; especially, he measured the weights which could be lifted by flying insects. Surprisingly, no application of his father's invention to the study of insect flight was reported until 1890 (*8*).

These three tools stimulated investigations into animal movement and led to the invention of moving pictures. This is one of the nicest examples of serendipity and should be much more widely quoted when the value of pure research is called in question. In view of the present magnitude of the film industry and the funds that were invested in the study of animal movement in the latter half of the last century, it may be aptly said that the mouse labored and brought forth a mountain. Horse racing should perhaps be regarded as the other parent of the film industry, since the investigations which led to the principal inventions on which it is based were prompted by a dispute between Frederick MacCrellish and Leland Stanford, governor of California, on whether a trotting horse ever had all four feet off the ground at a time. That it does was finally shown by E. Muybridge, who used batteries of cameras. His work culminated in the publication of 11 extremely rare but very illuminating volumes, *Animal Locomotion, an Electrophotographic Investigation of Consecutive Phases of Animal Movement,* in 1887. Marey and others with less funds at their disposal were stimulated by Muybridge's work to attempt and finally accomplish the same objective with single cameras —essentially the first movie cameras. Perhaps the film industry may one day be prevailed on to endow a laboratory for the further study of animal movement in token of its appreciation.

In the early days of photography not everybody was convinced that it could subserve the study of animal locomotion, and contributions continued to be made by direct observation. Poujade (*9*) recorded the attitudes adopted by insects in flight. Lendenfeld (*10*), Janet (*11*), and others elucidated some of the features of wing movement through morphological studies. At the turn of the century the general features of the wing movements of insects were known, and there were data on the frequency of these movements for a small number of species. Two problems stood out: How did the structure of the insect operate to produce the known features of wing movement, both spatially and temporally? How did these movements produce the lift and thrust forces required for flight?

The 20th Century

The 20th century opened with man's first flights. War then spurred the development of this skill but to the exclusion of advances in the study of animal flight. The two problems handed down as a legacy from the previous century received scant attention until World War I was over. The studies of Snodgrass (*12*) and others in functional morphology gradually resolved most aspects of the first of them, and we now know how all the normal movements of the wings in most groups of insects are accomplished.

The second problem fared worse. After many years of patient study, Magnan (*13*) came to the disturbing conclusion that on the basis of normal aerodynamic principles, a bumble bee should not be able to fly at all. His was the characteristic slow approach of the Old World. Across the Atlantic and perhaps equally typical of the New World, Townsend (*14*) had concluded, on the basis of brief field observations, that the deer fly *Cephenemyia* could travel at 820 miles per hour. This astounding claim took hold of the public with the fascination of the fantastic and may still be heard voiced today. It was not refuted, even in the sci-

Fig. 2. A reconstructed drawing of the fossil insect *Stenodictya lobata* Brongniart, showing the paranota, lateral extensions on the prothorax and on the abdominal segments (about one-half actual size). [Redrawn after Handlirsch *et al.*]

entific literature, until 12 years later when Langmuir (*15*) calculated from certain estimated quantities and approximate dimensions some of the improbable consequences which could be expected if the figure were true. Langmuir's procedure suggested to me that if the quantities and dimensions were accurately measured, it should be possible to predict the performance characteristics—speed, range, and endurance—of any insect and perhaps also to generalize from a sufficient number of species regarding insects as a whole.

The dimensions and quantities which had to be measured in order to do this were the energy available to the insect, the efficiency with which this could be transformed into useful aerodynamic work, and the power required for flight.

I was lucky enough to choose for my initial studies (*16*) a group of insects which use nectar as the principal fuel for flight and carry scanty tissue reserves—mosquitoes, blackflies and horseflies. The measurement of the energy available thus resolved itself into the measurement of the crop capacity of the insects and the concentration of the nectars available to them in the field. The energy values of the sugars contained in nectar are well known; concentrations proved higher than I had anticipated. The average energy value of nectar in the area of northern Manitoba, where this work was done, proved to be about one-third of that of the same weight of gasoline.

The over-all efficiency of a flying insect varies with the speed and may be broken into three parts: muscular efficiency, mechanical efficiency, and aerodynamic efficiency. It is simplest, however, to measure the over-all efficiency in one step. I did this by mounting the insects on small flight mills or ergometers (Figs. 3 and 4) and recording their performance on measured amounts of food fuel. Since no two individuals flew at the same speed, it was possible, from the results of these tests (Fig. 5), to draw curves connecting speed with fuel consumption and with efficiency.

The measurement of the power required for flight proved to be rather more difficult, largely because of the incompleteness of our understanding of the mechanism of insect flight. The power required can be divided into two parts: that for support or lift, to overcome the weight of the insect, and that for propulsion, to overcome the parasite drag (*17*) or wind resistance. The power required

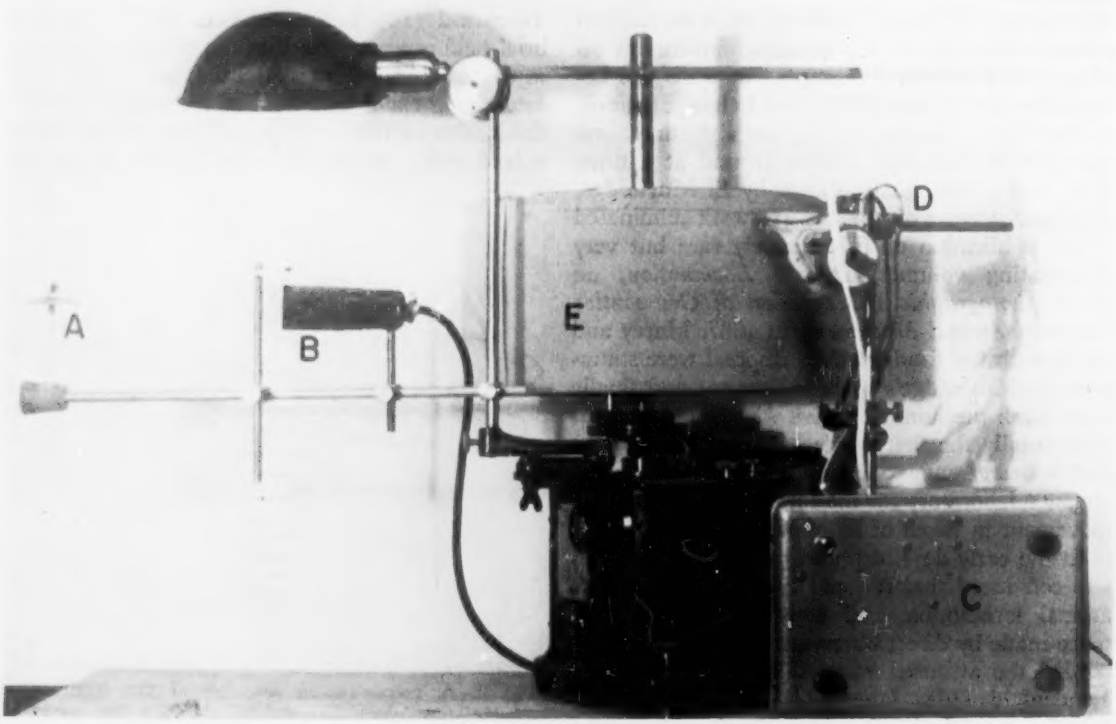

Fig. 3. A flight mill with amplifier and recording drum. (*A*) Insect on landing stage, (*B*) photocell, (*C*) amplifier, (*D*) time marker and stylus, (*E*) electrosensitive paper on kymograph drum. [From B. Hocking, *Transactions of the Royal Entomological Society*, vol. 104, pt. 8 (London, 1953)]

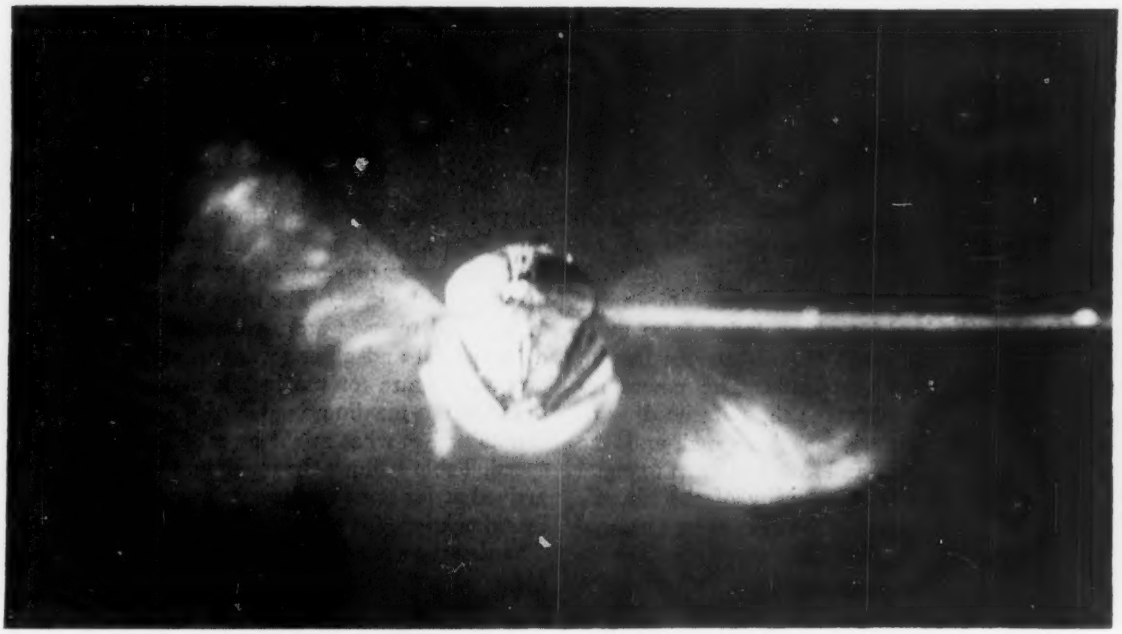

Fig. 4. A head-on view of the horsefly *Tabanus affinis* Kirby, flying on a mill (×4). [From B. Hocking, *Transactions of the Royal Entomological Society*, vol. 104, pt. 8 (London, 1953)]

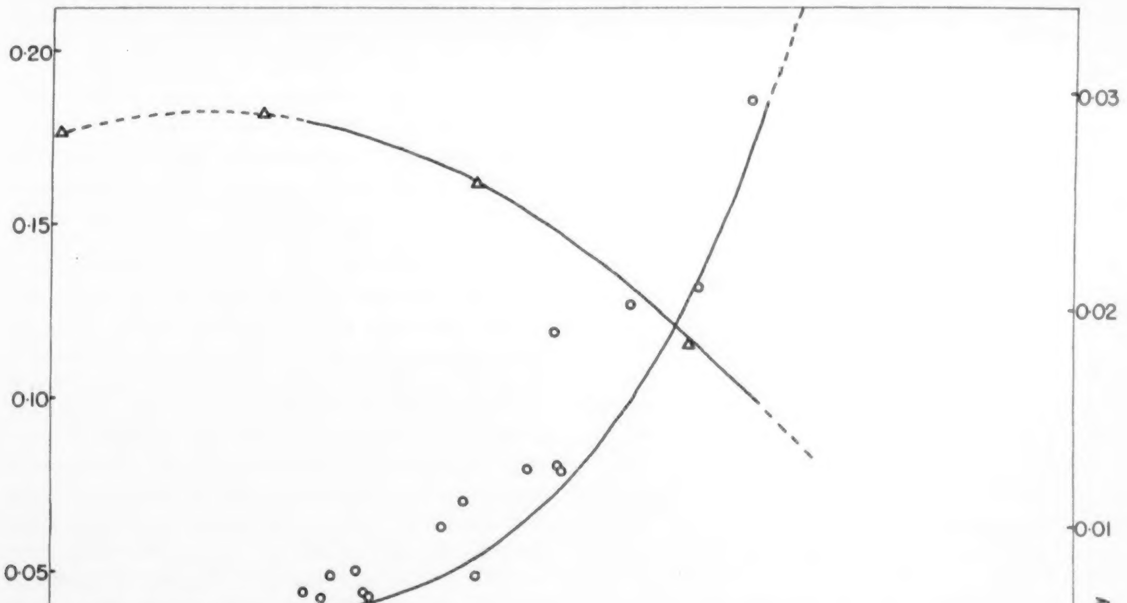

Fig. 5. The relationship between air speed, fuel consumption, and efficiency for the mosquito *Aedes impiger* (Walker). [From B. Hocking, *Transactions of the Royal Entomological Society*, vol. 104, pt. 8 (London, 1953)]

for lift depends, of course, on the mass of the insect and also on the amount of air passing through the wing system in unit time. It may be shown to equal

$$\frac{M^2g^2}{2m}$$

where m is the mass of air acted on by the wings in 1 second. I was able to measure the value of m experimentally for two of the larger species studied by mapping the slip-stream pattern (Fig. 6) with a hot thermopile anemometer. Values for other species were obtained by extrapolation from these figures and the parameters of wing action: length, area, frequency, and amplitude. The power required for thrust to overcome the parasite drag of the insect body depends on the speed and the dimensions of the body and may be shown to equal $\frac{1}{2}\rho C_D S V^3$ where ρ is the density of the air, C_D is the drag coefficient, S is the maximum sectional area, and V is the speed. The power required for each of these two needs is about the same for insects of average size at average speeds of flight.

When these quantities have been measured, it may be shown that the range of flight at any given

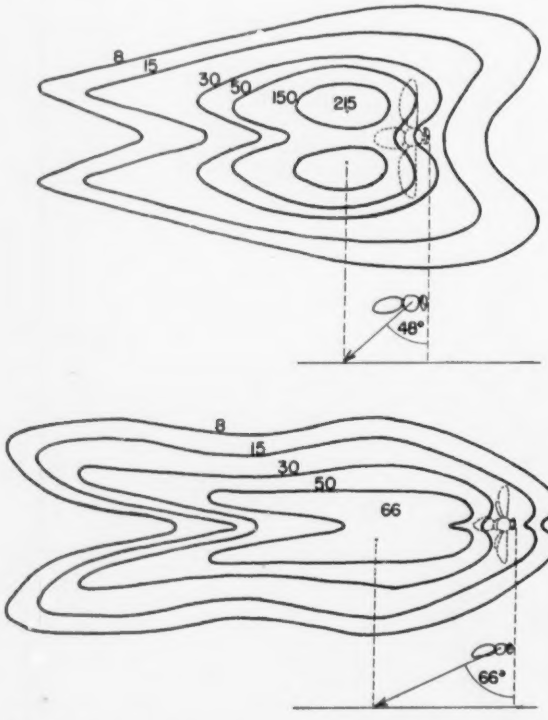

Fig. 6. The distribution of air speeds in the slip streams produced by the horsefly *Tabanus affinis* Kirby (top) and the honey bee. Speeds in centimeters per second. One-half actual size. [From B. Hocking, *Transactions of the Royal Entomological Society,* vol. 104, pt. 8 (London, 1953)]

speed is equal to the product of the energy available and the over-all efficiency, divided by the parasite drag plus the power required for lift divided by the speed, or

$$R = \frac{Ee_0}{d_p + M^2g^2/2mV}$$

where E is the energy available, e_0 is the over-all efficiency, and d_p is the parasite drag. Clearly, then, the range depends on the speed and also on the mass of the insect. The mass changes, of course, as the food reserves are used up, but it is possible by graphical and other means to arrive at an appropriate figure for use in this expression. It was found that a maximum value of the range of flight is obtained at what might be called a low cruising speed. Table 1 shows the maximum ranges of flight arrived at for some of the species studied and also, by calculation, for other species of special interest. The desert locust *Schistocerca gregaria* Forsk. was chosen because of its economic interest and because field data on its capabilities are available; the monarch butterfly *Danaus plexippus* L., because it appears to be particularly well adapted for long-range flight and is believed to have flown on occasion across the Atlantic. Both of these insects use fats as fuel for flight; this is advantageous in long-range flying, since fats provide about 3 times as much energy per unit weight as nectar does. Clearly a tail wind of quite ordinary speed would permit the monarch butterfly to cross the Atlantic.

In considering the maximum speed of flight, it is necessary to know the weight of the muscles used in flight, since this determines the rate at which energy can be transformed. In the insects examined this ranges from 16 to 35 percent of the weight of the body. The efficiency with which the insect can operate decreases with increasing speed of flight and approaches zero as the maximum possible speed is approached. It is well known that muscle can develop much more power per unit weight for a very short period than it can in continuous operation; this was well shown by direct observations on the speed of flight that were made on the flight mills used for efficiency measurements. I measured the drag due to the mill at various speeds and corrected for this. Speeds obtained in this way, as well as those obtained by calculation from the muscle weight, using the expression

$$V = \sqrt[3]{\frac{2P}{\rho C_D S}}$$

where P is the maximum power available after allowing for efficiency losses, are given in Table 1; these are all air speeds in level flight.

Field observations indicate that dragonflies are

Table 1. The maximum range of flight of insects, and air speeds in level flight.

Insect	Range (mi)	Speed (mi/hr)				
		For maximum range	Maximum continuous		Maximum in short burst	
			Observed	Calculated	Observed	Calculated
Horseflies						
Tabanus affinis Kirby	61	6.1	8		14.8	14.3
T. septentrionalis Loew	63	5.6	6.1	5.6	13.7	14.4
Chrysops furcata Walker	43	4.7		5.9		14.3
Mosquitoes						
Aedes campestris D. and K.	33	2.6		1.7		4.9
A. communis de Geer	14	2.7		1.8		5.6
A. impiger (Walker)	30	2.5	3.5		5.0	5.5
Blackflies						
Simulium venustum Say	72	2.8	4.3		5.3	5.8
Honey bee	29	5.6	7.5		9.4	13.9
Drosophila melanogaster Mg	30	2.2	3.8		4.1	4.3
Desert locust	217	5.6				
Monarch butterfly	650	6.2				
Dragonflies						
Austrophlebia sp.				24		36
Meganeura sp.						43

among our fastest fliers, and Tillyard (*18*) has reported that a species of *Austrophlebia* can fly at nearly 60 miles per hour, but this may have been in a dive with a tail wind. It can be shown that, other things being equal, the larger an insect is, the faster it is likely to be able to fly. The fossil dragonfly *Meganeura*, which had a wing span of 29 inches, could presumably fly faster than any species now living. Indeed, it probably had to, to remain air-borne at all. The speed shown in Table 1 is based on *pro rata* estimates of muscle weight and sectional area of thorax.

The Future

One of the outstanding difficulties encountered in this work was the lack of a complete understanding of how the movements of the wings produce the required lift and thrust. It seems likely that we shall have this understanding for at least one species, *Schistocerca gregaria* Forsk., before very long. Weis-Fogh and Jensen (*19*), a two-man team in Europe—insect physiologist and aerodynamics engineer—have published the first four of a series of ten papers dealing largely with this topic. When all ten parts of this work are published, an understanding of the essential features of wing movements and how they accomplish flight for any insect will be possible. It is my hope to extend certain aspects of their work, especially those dealing with energy relationships, to other insect species, covering as wide a size range as possible, from *Drosophila* upward.

At the heart of the problems of insect flight there is still the question of the action of wing muscles. The work of Sotavalta (*20*) has extended our knowledge of the frequency of wing beat. We now know that this may be as high as 1000 cycles per second, which means a complete muscular contraction and relaxation in 1 millisecond, as compared with the normal time required for this operation in vertebrate muscle, about 0.1 second. This peculiar property of flight muscle is correlated with peculiar histology and, as Pringle (*21*) and Roeder (*22*) have shown, with peculiarities in nerve stimuli also. Such stimuli may be transmitted to the muscles only at intervals of several cycles. The stimuli for the intermediate contractions have been supposed to come from the stretching of the muscles by their antagonistic muscles; or it may be that they are simply springlike oscillations and are not "biologically powered." Complete clarification of the mechanism of rapid action of the flight muscles is likely to take some time; indeed, we have some way to go before we have an adequate understanding of the action of more ordinary muscles.

With this core of basic information to work with, it should then be possible to tackle some of the more difficult problems—those related to control and maneuver especially; it is here in particular that animal flyers begin where aircraft leave off, and it is here that aeronautics may expect to profit from biological studies. Von Holst and Kuchemann (*23*) have described a model aircraft, which might be called an "entomopter," modeled on a dragonfly. This had remarkable performance characteristics.

Contributions seemed likely to be greatest in the theoretical field, however, at least until the mechanisms of insect maneuver begin to be understood. Perhaps the problems related to the flight of very small insects will prove fruitful in the realm of aerodynamic theory. Most of these insects, although they are quite unrelated, have strikingly similar narrow wings with broad fringes of hairs; their flight has been discussed by von Holst and Kuchemann and more recently by Horridge (*24*).

Some of the simpler problems to begin on are the mechanism of steering (both horizontally and vertically) and speed control. How is the speed of flight related to frequency and amplitude of wing beat? How is it that hover flies can fly backward and sideways? Just what happens at take-off and landing? Is it true that some flies can fly upside down, and if so, how is this accomplished? What determines the direction, speed, and duration of a flight, and how do the sense organs function during this activity?

Nobody has progressed far in the study of entomology without becoming aware of the importance of the variations in venational patterns in the wings. This has nearly always been looked at in the light of its phylogenetic and taxonomic significance, and functional significance has been ascribed only to such generalities as the concentration of veins toward the anterior margin. It seems quite likely that, as we learn more about insect flight, we may find functional significance in many other features of wing venation. Students who have struggled with the complexities of vein homologies, real and imaginary, should welcome this.

To look further into the future and, at the same time, to return to our initial topic, evolution—what can man expect of insects and their wings in the future? Most people are familiar with the fact that the widespread use of insecticides has wrought some rapid and disturbing evolutionary changes in insects. These already include changes in flight behavior and are likely to include more. Changes in flight performance are likely to follow. We know too little of the interaction between insects and animals other than man to guess further.

References and Notes

1. C. Gegenbauer, *Grundriss der vergleichende Anatomie* (Leipzig, 1870–78).
2. F. Müller, *Jena. Z. Naturwissenschaft* **7**, 337, 451 (1873–75).
3. B. B. Rodendorf, *Trav. inst. paléozool. acad. sci. U.R.S.S.* **16**, 1 (1949).
4. W. T. M. Forbes, *Am. Midland Naturalist* **29**, 381 (1943).
5. See J. A. Chapman, *Nature* **177**, 1183 (1956).
6. G. A. Borelli, *De Motu Animalium* (Rome, 1680), vols. 1, 2.
7. For this and other references to the history of photography, see H. Gernsheim and A. Gernsheim, *The History of Photography* (Oxford Univ. Press, London, 1955).
8. E. J. Marey, *Le vol des oiseaux* (Paris, 1890).
9. G. A. Poujade, *Ann. soc. entomol. France* **4**, 197 (1884).
10. R. von Lendenfeld, *Proc. Linnean Soc. N. S. Wales* **9**, 986 (1885).
11. C. Janet, *Compt. rend. acad. sci. Paris* **128**, 249 (1899).
12. R. E. Snodgrass, *Proc. U.S. Natl. Museum* **36**, 511 (1909) and other papers.
13. A. Magnan, *La locomotion chez les animaux* (Paris, 1934).
14. C. H. T. Townsend, *Sci. Monthly* **13**, 309 (1926).
15. I. Langmuir, *Science* **87**, 233 (1938).
16. This work was financed by the Defence Research Board of Canada and was reported in full in *Trans. Roy. Entomol. Soc. London* **104**, 223 (1953).
17. An aerodynamic, not entomological, parasite: the minimum drag on the insect body without wings.
18. R. J. Tillyard, *The Biology of Dragonflies* (Cambridge Univ. Press, London, 1917).
19. T. Weis-Fogh and M. Jensen, *Phil. Trans. Roy. Soc. (London)* **B239**, 415 (1956); T. Weis-Fogh, *ibid.* **B239**, 459, 553 (1956); M. Jensen, *ibid.* **B239**, 511 (1956).
20. O. Sotavalta, *Ann. entomol. fennica* **7**, 32 (1941); *Acta entomol. fennica* **4**, 3 (1947).
21. J. W. S. Pringle, *Phil. Trans. Roy. Soc. (London)* **B233**, 347 (1948).
22. K. D. Roeder, *Biol. Bull.* **100** 95 (1951).
23. E. von Holst and D. Kuchemann, *J. Roy. Aeronaut. Soc.* **46**, 39 (1942).
24. G. A. Horridge, *Nature* **178**, 1334 (1956).

Vitalistic-Mechanistic Controversy

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THE ultimate hazard for any form of protoplasm is the possibility that it will fail to reproduce its kind. Although the one is compounded of the other, the death of a race or species is an event on an altogether different scale than the death of an individual organism. Death is inevitable for an individual but not for any species. Yet, in every case, the death of an organism or species marks the end of an evolutionary experiment involving the ability of a set of hereditary factors (genes) to utilize the substance of an environment in perpetuating its kind.

Darwin stated the facts rather graphically: "For we have reason to believe that only a few species of a genus ever undergo change; the other species becoming utterly extinct and leaving no modified progeny" (*1*). Any species survives only if its supply of environmental resources is not significantly reduced during the existence of succeeding generations. Just as cancerous tissue survives only as long as its environing host organism, so too a species survives only as long as its environment remains fit. Insofar as man can alter his environment, to that extent also, he has the ability to destroy its fitness for his survival as a species. The risk of extinction, then, may be commensurate with the degree of ecological dominance achieved by any race or species. The continuation, then, of human culture depends upon man's capacity to survive the rigors of natural selection on earth.

Ironically, man's competition with other species on earth is now overshadowed by an even more intense struggle among men. This intraspecific struggle is essentially of two types: intermittent armed warfare and a more widespread, continuous conflict of ideas. The two types, of course, are interrelated, and either one may lead to the other and to a major catastrophe. Although they overlap, and occasionally may be pursued conjointly, we probably never can have an armistice in the intellectual war. This conflict is present within, as well as among, all races and nations. The biological, cultural, economic, political, and psychological factors that lead to strife among men have been analyzed and defined by many. But mostly, scholars have stressed the evils of armed conflict, and, in my opinion, they have underestimated the dimensions and consequences of the ideological conflict, especially within nations. The possibility of peace and happiness rests just as much on the mitigation of the one kind of conflict as it does on the mitigation of the other.

However, our present ecological eminence cannot be maintained by resting on our laurels. Our increasing rate of exploitation of the natural resources of the earth poses problems which cannot be solved as easily as were the problems of subjugating organisms with less intelligence than ours. Overpopulation, diminishing supplies of critical raw materials, waning agricultural acreage—these problems together are as dangerous as atomic or thermonuclear warfare, if not more so. Human beings will have to exercise their utmost abilities to meet these crises within a few generations.

All these problems are the inevitable by-products of man's biological superiority on earth. They are mute evidence that humanity has not yet achieved even an approximation to that system of environmental relationships which is necessary to insure his survival as a species. But from an evolutionary perspective, the will to know and understand is even more critical than either physical destruction or the eventual supremacy of any ideology. The ruins of civilization can be reconstructed, whereas the loss of the genetic basis for human intelligence would destroy not only civilization but also man's ability to reconstruct it. Without doubt, man achieved his status on earth by means of his conscious control of behavior. But if his thinking creates destructive forces which are uncontrollable, then mentality itself represents a malfunction of the nervous system, and man will disappear from the earth.

Scientific Perspective

Natural science represents our most efficient means of knowing the universe. But scientists are in a minority in any society, and they are helpless, as are any leaders in a democratic organization,

without the active support of a significant number of individuals. Already, the impact of scientific inquiry on civilization has been both drastic and profound. The beneficence of modern medicine and the local abundance of products in industry and agriculture justifiably are occasions of great pride and satisfaction. But there have been occasions of great sorrow also, for the impact of science upon the mode of war has been drastic and profound. And, as is noted by Otto, "a good many people who should be able to make a wise choice—college professors, men of letters, and religious leaders—do not agree that what is needed is more science, a great deal more science" (*2*). Perhaps the notion of a moratorium among scientists has arisen from the widespread illusion of a "spontaneous generation" of truth. Surely a critical analysis of recorded history refutes the wishful thought that ignorance and prejudice inevitably must give way to beneficial progress. An original insight must be verified in the experience of many persons before its establishment as fact is warranted. Truth is a social as well as a private event, and social evolution is not influenced by uncommunicated ideas. The spread of knowledge is accomplished by transactions among people; it is not an autonomous process and therefore is not inevitable.

The supernaturalistic and antiscientific ideas which still dominate so many of our social amenities are often clearly irreconcilable with scientific theory. The paradox is that the material aspects of our existence are operated largely in accordance with the dictates of research, but, outside the scientific fraternities, the ideological aspects of our existence are still preponderately governed by dogmatic, unscientific concepts. "Nothing is more dramatically conflicting," wrote Frank (*3*, p. 142), "than the recently developed modes of thinking and action in science and technology and those used in social and personal living where we cling loyally to anachronistic, even archaic patterns and stubbornly resist the kind of critical thinking and testing that have made science and technology possible." Frequently, persons who call themselves scientists are the best possible examples of this "schizophrenia." The inevitable tensions which arise from this bizarre intellection cannot be sublimated forever.

Presumably, the alternative to reasoned control of behavior is instinct or nonrational response. But this is not an alternative for man. To renounce the use of the only superior weapon he has in the struggle for existence amounts to racial suicide, or to a return to the status of man's primate ancestors before their development of communication and their use of tools. Thus the prospect of extending scientific habits of thinking to all phases of existence remains a tantalizing vision with revolutionary implications for the betterment of mankind. But, even intellectual revolutions involve some disruption of the *status quo*. Understandably, the revolutionary implications of scientific advance incur the distrust and even the determined opposition of some who would preserve the traditions and forms of society in which they have vested interests.

These antagonists of the extension of scientific inquiry into the humanities confuse the stability of symbols with security. They associate the ephemeral aspects of scientific theory with ephemeral moral standards (*4*). They cling to static concepts of science, not only because of their inadequate background and training, but also because, for them, the problem of maintaining an autonomous realm of authority in society is simplified in their own minds if their concepts of science are emasculated and anemic. Foremost among the antagonists of science are the religionists of supernaturalistic faiths and the philosophers of dualistic or idealistic convictions. As is stated by Otto: "A concerted campaign is being conducted . . . to make the scientific attitude the scapegoat for present evils, while the return to the beliefs and practices of a prescientific and pretechnological age is urged as the road to our salvation. Even scientists are among those who have succumbed to this reactionary mood" (*2*).

Thus, in spite of a myriad of material benefits already at hand, many Americans and Europeans still suspect scientists, as a group, of being somehow morally derelict and irresponsible. And so, despite the penetration of science into all niches of society, its potential influence for progress has waned amid fear and distrust. As a consequence, the battle lines are drawn between the advocates of scientific inquiry (not all scientists) as a universal method of solving problems and their opponents (not all nonscientists), who would virtually destroy science as a social force by divorcing it from values and morality. The stakes are high, no less than survival versus possible extinction.

None of this is meant to deny the importance or value of such truth as might be embodied in a great painting or musical composition. But I would insist that this truth has a different quality and function from that of empirical science which is subject to verification by controlled experimentation in which the anthropocentric factor is reduced to a minimum. Because of its operational and pragmatic basis, scientific truth is more easily verified among persons of different cultural backgrounds and, therefore, its potentiality for universal

communication, understanding, and brotherhood is unique. Admittedly, art and philosophy can be very important aids in this progression, but they cannot serve as satisfactory substitutes for science.

As is suggested by Sinnott (*5,* p. 12): "The area to be explored is a domain into which many have been tempted to rush without due preparation—and thus have met with disaster—and which others find so full of snares and pitfalls that it is impossible to penetrate. He who enters it will always be in danger of violating one or another vested interest in science, philosophy, or religion and must therefore be either a bold man or one no longer vulnerable to the perils that lurk in being heterodox." However, among scientists, heterodoxy has a long and respectable tradition. In fact, one could say that heterodoxy is almost a synonym for the scientific attitude in its greatest moments, and, indeed, this is precisely why the supernaturalists and authoritarianists so stubbornly oppose the introduction of science into their carefully guarded "metaphysical" citadels.

But, in accordance with the oldest and noblest traditions among them, scientists share the collective obligation of self-criticism as well as the duty to communicate their discoveries to all receptive elements of society, for, without their testimony, the misapplication of scientific information to religious and philosophic issues may lead to an even greater schism than that which already exists between those dedicated to truth and a large mass of people who are confused by ignorance, myth, and superstition. This social obligation necessitates a radical change of thinking habits among those who are trained in the "ivory-tower" tradition of scholarship.

Historical Perspective

One of the major controversies in the history of biology involves the interpretation of protoplasmic functions. The alternative theories which long held the stage are vitalism and mechanism. Contemporary historians credit Aristotle with having formulated the idea of a "vital force" which he designated as "entelechy" or "psyche." According to Singer (*6*), the mechanistic view arose from the principles proposed by Descartes (1596–1650), "yet the details of the mechanism which he proposed were entirely imaginary and had no real existence." Singer maintained that the opposition between the mechanist and the vitalist came into view in the last quarter of the 17th century. As prototypes of those opposing views, he suggested the rival professors at Halle, Georg E. Stahl (1660–1734) and Friedrich Hoffman (1660–1742). At the height of the popularity of mechanistic ideas, living organisms often were compared to machines. Today there is agreement that, as Alexander (*7,* p. 21) phrased it, "the kind of machine that would have to exist to provide an analogy with a living thing" does not exist.

However, confusion arises from oversimplified espousals of mechanism like that of Villee (*8,* p. 10): "Almost all present-day biologists agree that the manifold aspects of living things are ultimately explainable in terms of the same chemical and physical principles as nonliving things." Taken literally, this resembles the proposition that all the properties of water are ultimately explainable in terms of principles derived from the study of hydrogen and oxgen of which water is composed.

The brief statement by Dobzhansky (*9,* p. 19) is more adequate: "Most biologists believe that the best working hypothesis is that life phenomena involve merely complex patterns of interaction of physical forces and of chemical reactions. This assumption is called *mechanism.* Its alternative, *vitalism,* is that, in addition to the forces similar in kind to those operating in inanimate nature, life involves powers which are restricted to the living world."

During the first wave of enthusiasm for Darwin's theory of organic evolution by means of natural selection in the latter half of the 19th century, vitalistic doctrines came into disrepute among scientists. However, as early as 1894, Hans Driesch (1867–1941) was bold enough to announce the death of Darwinism and to propose his own interpretation of the nature of protoplasm and of the process of organic evolution. So convincingly did Driesch argue that Radl, an eminent historian, wrote in 1930 that "Driesch marks the end of Darwinism" (*10*). As it happened, at the turn of the 20th century, the science of genetics became the focus of attention of most biologists. Rather than supplanting the theory of evolution by means of natural selection, as a few biologists supposed at the time, evidence from genetic research came to supplement Darwinian theory. Consequently, at least among scientists, Darwinism has more support today than at any former time in its history.

Another important development in the controversy was the appearance of Bergson's theory of creative evolution in 1907. Most contemporary philosophers have considered Bergson's *élan vital* as no more than a semantic variation of "vital force," although Bergson explicitly claimed to "transcend both mechanism and vitalism" (*11*). White summed up Bergsonism as follows: "For Bergson, living is a far more basic process than knowing. Life is an unceasing, continuous, un-

divided process, a sort of cosmic movement of which we are expressions rather than parts. It, rather than mind or matter, is the fundamental reality" (*12*). Something of the subsequent influence of Bergsonism can be gleaned from the lengthy account of it in the *Encyclopedia Americana*. Here it is appraised as "at once an explanation and a refutation of mechanism" (*13*). Unfortunately, it is not the agnosticism of an open, evolving mind which later philosophers derived from Bergsonism but, rather, an antirational bias and reliance on intuition as the mainspring of mentation.

Telefinalism and Telism

More recently, the books of Lecomte DuNoüy and Edmund W. Sinnott have received considerable publicity in the popular press, probably because their contents are interpreted as scientific support of supernaturalistic or spiritualistic doctrines. The opinions of these two men deserve attention because of their sales appeal as well as because of the eminence of both men as scientists.

On the other hand, the views of a majority of scientists, expressed in such books as *Man and His Gods* by H. W. Smith (*14*) and *The Unleashing of Evolutionary Thought* by Oscar Riddle (*15*), are unnoticed or even deliberately ignored, as is implied by Sears in his review of Riddle's book (*16*). The possibility is that many laymen will mistake the opinions of DuNoüy and Sinnott as representative of scientists generally. I submit that their opinions are not so representative.

Both DuNoüy and Sinnott apparently inherited one basic assumption from their vitalistic forebears. They accepted an extrascientific principle, which they named "telefinalism" and "telism," respectively, and which they believe controls or directs causal systems in some mysterious manner. DuNoüy was explicit: "At present there is no hypothesis capable of explaining the birth of life and the development of consciousness without the intervention of a factor that can be described as extrascientific or supernatural" (*17*, p. 189). Similarly, Sinnott wrote: "The Principle of Organization is far more than a scientific concept. It states a belief that there is operating in the universe a something that leads to spirit; something that is spirit" (*5*, p. 169). These quotations represent, as Mayer put it, "finalistic views in which materialism and therefore too, the ultimate adequacy of scientific inquiry is expressly repudiated" (*18*, p. 47). Obviously, as Simpson judged: "All of these theories, vitalist, finalist, or both, involve some degree of abandonment of causalism. . . . Once causalism is abandoned there are no limitations on flights of imagination. . . ." (*19*, p. 131).

Clearly enough, the basic issue is not the nature of protoplasm but rather one of different feelings about the nature and function of explanation and inquiry. In Burtt's view of modes of explanation: "there would seem to have been to date three basically distinct convictions on this matter. One is the teleological position of Platonic and Aristotelian philosophy . . . that the cause must be adequate to the effect. . . . When worked out in detail, this means an essentially religious picture of the world, and a being not dissimilar to the scholastic deity must be postulated as the ultimate and all-embracing cause of events. The second is the mechanical position . . . that all causes and effects are reducible to motions of bodies in time and space and are mathematically equivalent in terms of the forces expressed. . . . The third position is the evolutionary one . . . the increased feeling that the phenomena of growth . . . require a type of causal explanation essentially different from either of the foregoing. The central assumption of this position is that the cause may be simpler than the effect while genetically responsible for it" (*20*, p. 308). From the perspective provided by Burtt, the irresistible inference is that telefinalism and telism are to be classified as mental stepchildren of teleological explanation.

According to Newman, Anaxagoras (500–428 B.C.) was the first of the Greeks to "attribute the adaptations of Nature to Intelligent Design, and was thus the founder of Teleology" (*21*). That master of analysts, Aristotle, nevertheless, must be credited with first clearly distinguishing between materialistic causality and purposiveness. But there is nothing to be gained here by tracing the development of teleological thought from the time of the Greeks to the present.

DuNoüy called his hypothesis "telefinalistic," which "presupposes a distant goal but admits different methods for the attainment of this goal" (*22*, p. 18). Sinnott named his theme "telism, the philosophy of goals—a belief that what is important is not the push and drive of a living system but the drawing power of a goal, conscious or unconscious, that in some way is established in it" (*5*, p. 120). Guyer summarized such views very simply: "They [the vitalists and finalists] believe they find evidence of purpose in life activities and that such activities are inexplicable on the basis of mere chemistry and physics" (*23*). Pragmatically, there is no difference between a purpose which precedes and pushes matter toward some predestined goal and one which has the "drawing power of a goal," as conceived by Sinnott. In both

"purposes," the appeal to an extrascientific principle precludes any further inquiry into the nature of protoplasm. Sinnott, himself, professed that many biologists will be offended by his support of the ". . . reality of purpose in living things and thus of teleology" (*5*, p. vii).

The universal fallacy of teleology is the reification of mental concepts. As Korzybski would have said, this fallacy involves the confusion of words with things. We must distinguish between imaginary concepts with subjective reality only and objective concepts supported by scientific evidence. The same word may serve as a designator for both kinds of phenomena. A judgment about the quality of the two kinds of phenomena depends on the kind of evidence that can be marshaled around them. Some of this confusion may have its lineage in the formulation of final cause by Aristotle; it has been perpetuated by the indiscriminate usage of the question "why" for information about materialistic causes as well as about conscious reasons for human behavior.

Teleological explanation is an abandonment of the means of inquiry which enabled man to attain his exalted position in the animal kingdom. A teleological argument is a confession of the acceptance of an extra-scientific principle to explain the existence of purposeful behavior or conscious intelligence. Its usage marks the end of inquiry into the natural origin of any phenomenon. Reichenbach (*24*, p. 202) concluded that "Life can be explained along with all other natural phenomena, and biology requires no principles that violate the laws of physics. The apparent teleology of living organisms is reducible to causality." Similarly, Mainx opined that "the attempt to make a distinction between efficient and final causes seems meaningless without an anthropomorphic psychologizing of natural processes" (*25*, p. 640).

Once we accept a natural origin of purpose, little mystery remains. Foresight is based on memory of alternative courses of action and their probable results. If mechanisms such as magnetic tape recorders and electronic brains are capable of remembering, certainly a materialistic basis for their human analogs is not inconceivable.

If we accept vitalism, telefinalism, and telism as belonging to the teleological mode of explanation, we may then consider the suggestion made by Holmes (*26*) that "Historically, the doctrine [vitalism] is a lineal descendant of the animism of primitive man." LaBarre explored the implication of this inference: "The belief in animism is a moral and intellectual infantilism of man—a dependent clinging to what is archaic in the individual as well as ancient in the race" (*27*, p. 283). "For just as the individual neurotic still clings to the false beliefs and symbol-identifications of his past that contradict his present experience, so too do societies of men hold to the outmoded beliefs of the past . . . when other and better explanations are long since available" (*27*, p. 302). As Mayer concluded (*18*), "Minds not developed by scientific discipline will always go on searching for final causes. Their demand for such an explanation will lead them toward idealism and to the imagining of commanding myths which will satisfy their craving to believe. That signifies nothing but a renunciation of their reason in the face of certain problems."

Another feature common to vitalistic doctrines is an insistence that scientific explanations of organic evolution and protoplasm are inadequate. One could hardly deny that there is some truth in this claim. Nevertheless, the specific arguments offered to support it appear to fall in the category described by Mainx (*25*, p. 630) as "a special, perhaps intentional, lack of understanding of the results of genetics and their connections with the achievements of developmental physiology." The general bases for these arguments are chance, determinism, thermodynamics, the origin of life, and the process of organic evolution.

DuNoüy's argument from considerations of probability is typical: "It is impossible, because of the immense complexity of the problem, to lay the foundations of a calculation that would establish the probability of the spontaneous appearance of life on earth. However, we can approach this by simplifying the problem. . . . The probability that a single molecule of high dissymmetry can be formed by the action of thermal agitation is practically nil" (*17*, p. 123). That this argument misses the point was pointed out by Cohen: "For even if we were to grant the validity of its mathematical reasoning, the latter could prove nothing more than that in a chance universe spread through space and time, the occurrence of life should be extremely rare. That, however, is precisely the actual case" (*28*, p. 212).

Furthermore, Mainx pointed out that "Structures which are improbable when considered from the point of view of inorganic models are formed by the special biochemical and biophysical situation in the organism and are thus very probable" (*25*, p. 633). If we follow DuNoüy's reasoning, we would have to conclude that any sequence of heads and tails, established by tossing a coin a very large number of times, is impossible. The longer the series which is postulated, the less is its probability of occurrence. Yet, in reality, one such series, if we are willing to toss the number of times required and to record the results, must happen. In this statis-

tical sense, the highly improbable occurs regularly and indeed is inevitable.

The fallacy in the argument based on determinism is almost identical. DuNoüy wrote: "Causal relations must then exist between the atoms. . . . If this is not the case, integral determinism is . . . nonexistent, so to speak. . . . the concept of rigid determinism which was already shaken by the introduction of the calculus of probabilities as a method of interpretation is defeated by Heisenberg's principle of uncertainty, or indeterminancy" (*17*, pp. 175–178). This observation presumes that a principle must apply universally, whereas all scientific theories are conditioned by, and must be in accordance with, the specifications of an operational system. If a theory is tested by events in a different operational system, its failure means only that the theory has limited applicability. In science, principles are always subservient to facts, but not to all facts, only to those with which they have necessary connections.

DuNoüy also adduced arguments against mechanism from biological data: "Pure mechanism would be refutable . . . if it could be proved that life may manufacture the like apparatus by unlike means on divergent lines of evolution" (*22*, p. 54). As an example supporting this idea, he mentioned the common development of certain organs by the dissimilar first cells of a mollusk and a vertebrate animal. But, as we shall see, "pure mechanism" is an intellectual position of little consequence now and of even less applicability to biological theory of the mid-20th century. As a matter of fact, no modern evolutionist known to me maintains that parallel, or even convergent, evolution is valid evidence for or against either a mechanistic or vitalistic hypothesis. DuNoüy's argument overlooks the essential facts that an organism develops by means of its environment, to borrow a phrase from John Dewey, and that no consideration based on an artificial separation of the organism from its environment can be conclusive.

The third type of argument for the inadequacy of biological theory, that based on the laws of thermodynamics, varies from the simple affirmation that life contradicts those laws, to the artful appeal to the imagination posed by DuNoüy: "Can we not conceive of two degrees in the universe? The first corresponds to the material world, the realm of energy, of the first and second principles of thermodynamics, of macroscopic determinism, leading to more and more probable states of equilibrium. The second corresponds to the world of spiritual forces, bringing only minute quantities of energy into play but capable of orienting indetermination in a definite direction and no longer only 'in the most probable directions'" (*17*, p. 235). But a theory which has no other basis than speculation serves a useful function only if it leads to hypotheses which can be tested experimentally or by observation. This is merely a restatement of the inductive basis of science and of the principle of Occam's razor. Logic is a scientific tool when applied in the analysis of empirical data, but it can never serve as a substitute for data.

Biophysicists find no contradiction to the second law of thermodynamics in the physiological functions of protoplasm. Blum concluded that, "No matter how carefully we examine the energetics of living systems, we find no evidence of defeat of thermodynamic principles, but we do encounter a degree of complexity not witnessed in the non-living world" (*29*, p. 119). Mainx supported this conclusion (*25*). Again, the fallacy involved is one of considering an organism as a closed system, one apart from its environment, when in fact potential energy continually enters the organism.

Sinnott avoided any open contradiction of thermodynamic principles by assuming that the "psyche" which, supposedly, controls the directiveness of organisms is an inherent characteristic of protoplasm. But, as Holmes implied (*26*, p. 14), and, as Dobzhansky stated, "If these factors somehow direct the flow of physical and chemical processes, they must themselves be forms of physical energy, even though peculiar and as yet unknown ones" (*9*, p. 19). History is replete with examples of imaginary concepts, such as phlogiston and the ether of outer space, which, although apparently useful temporarily, in their day delayed scientific progress. Ruthless surgery with Occam's razor is the best remedy for them.

DuNoüy probably was not, and Sinnott still may not be, aware of recent developments in biochemistry and of trends in evolutionary theory, for Sinnott declared that "whether or not life arose by anything comparable to natural selection . . . we cannot say, but this seems very doubtful" (*5*, p. 107). To my knowledge, none of the outstanding geneticists or evolutionists doubt the credibility of the origin of protoplasm on earth in a process analogous to natural selection among organic compounds as first outlined by Oparin in 1936. The process, Oparin wrote (*30*, pp. 250–251), involved "Competitive speed of growth, struggle for existence, and finally, natural selection . . ." and "determined such a form of material organization which is characteristic of living things of the present time." Since Oparin's book appeared, a number of scientists have expressed themselves explicitly on this point.

Blum wrote: "That life was 'spontaneously' gen-

erated from nonliving matter at some time in the very remote past, and that this process has not been repeated for a long time, are two basic tenets accepted by the great majority of biologists. The first idea . . . seems a necessary part of the evolutionary concept." With regard to how this was possible, "the general answer is that the conditions no longer exist which once made the spontaneous generation of life possible. . . . Oparin suggested that under existing conditions incipient new life, or its building materials . . . are destroyed before new life has an opportunity to develop, because of the 'predatory' activity of already existing life" (*29*, p. 173). Wald (*31*) also was explicit in his support of Oparin's hypothesis. The most recent contribution to this discussion is Calvin's (*32*): "we can conceive of a continuous process, beginning with a bare earth . . . leading to the random formation of more or less complex molecules, then gradually, by this process of random variation, autocatalysis and selection to more complex systems, and eventually to the ordered arrays . . . which even today are the units that carry the continuity and order of present day living systems."

Admittedly this is not to say that the precise chain of molecular reactions from which life first arose will ever be established. In the nature of things, "proof" will be impossible forever. However, as Reichenbach (*24*, p. 201) remarked: "The indirect evidence we have for the evolution from the amoeba to the human being is so good that it scarcely needs an amplification by direct experiments."

Sinnott cast doubt on the validity of natural selection in the evolution of highly complex organisms: "The ability to regenerate lost parts is difficult to account for through natural selection since in most species a need for it would rarely be met," and second, "the specific shapes of tissue and organ so characteristic of living things which are the obvious result of organized and regulated growth seem rarely to be of value in evolution" (*5*, p. 168). But it seems to me that no matter how rarely a species might need to regenerate one of its parts, the ability to do so is a very important one. Furthermore, the ecology of predaceous animals and their prey indicates that many of them do have opportunity to regenerate lost parts. In fact, regeneration is a common feature of the life of plants and most animals, and often plays a vital role in the propagation of many of them. The difficulty of understanding its origin is no more complex than that of many other characteristics and presents no unique problems.

Sinnott's second objection is difficult to interpret but supposedly refers to nonadaptive structural features found in many organisms. Inasmuch as there is abundant evidence to show that mutations are usually harmful, sometimes neutral under certain conditions, and only occasionally beneficial, it is not surprising that some nonadaptive characteristics, if they are linked in their inheritance to other characteristics which are beneficial, should be reproduced indefinitely. As long as random variation is the rule among organisms, the existence of nonadaptive features is entirely natural. Even if we were to grant that Sinnott's examples are factual, they would establish nothing more than that present-day evolutionary theories are imperfect; most scientists would assent to that without embarrassment.

DuNoüy sought to discredit Neo-Darwinism by differentiating between evolution and transformation (adaptation) as follows: "Whereas adaptation blindly tries to attain an equilibrium which will bring about its end, evolution can only continue through unstable systems or organisms. . . . Adaptation then works to neutralize its own anterior efforts, and natural selection tends to eliminate those it had heretofore protected" (*22*, pp. 90–91). Having adopted this hypothetical dissociation of adaptation from evolution, DuNoüy believed that he had sidestepped the most important objection raised by the evolutionists against any finalistic hypothesis—namely, the innumerable and well-documented instances of extinction of once widespread kinds of plants and animals. From his point of view, "this was nothing but a negligible accident amongst thousands of others, inasmuch as these animals played *no part in evolution*, and represented only the tail end of a strain long before separated from the evolving system" (*22*, p. 92).

As a matter of fact, adaptation is an important part of evolution. Because hereditary adaptations, even within the population of a single species, may lead to death for some individuals and to survival for others, I see no justification for trying to eliminate either of these types of adaptation from the over-all process of evolution. If modern theorists are correct, evolution is a process in which the relative frequencies of different kinds of genes within a breeding population change. It is precisely the death of certain individuals or populations of a species which changes the frequencies of genes in the surviving individuals of that species.

The same set of genes, furthermore, under one group of ecological conditions, may not survive to reproduce, whereas under a different group of ecological conditions it may reproduce in large numbers. The separation of adaptation from evolution appears to me to be unwarranted. At any rate, to say that the development of a species over a period

of a million years is a "negligible accident" is the height of presumption. The earth itself, and man with it, may one day be such an accident.

Simpson (*19*, p. 131) had this to say of vitalists and finalists: "They did not explain evolution, but claimed that it is inexplicable and then gave a name to its inexplicability: 'elan vital' (Bergson), 'cellular consciousness' (Buis), 'aristogenesis' (Osborn), 'nomogenesis' (Berg), 'holism' (Smuts), 'hologenesis' (Rosa), 'entelechy' (Driesch), 'telefinalism' (DuNoüy)—the list could be greatly extended." Now we may add to this list, "telism" (Sinnott).

Philosophic Perspective

Philosophically speaking, Sinnott's views are more than orthodox; they are reactionary, for he would have us limited to the horns of a dualistic dilemma, an alternative of either traditional idealism or materialism. He wrote: "In the inevitable choice that each of us must make between a philosophy founded on belief in spirit as the supreme reality and one that finds reality only in tangible and measurable things, decision comes more often from unreasoned preference than from rational argument. . . . Whatever the truth in these deep matters finally turns out to be, we may conclude, I think, that spirit is reality" (*5*, p. 168). Most philosophers would list more alternatives such as Dualism, Naturalism, Pantheism, or Humanism from which a choice could be made. Indeed, most scientists do not espouse strongly any one of the classical philosophies. However, if one had to classify their opinions, almost all of them would be found to have been compounded of postulates derived from the naturalistic or humanistic schools of thought.

But Sinnott discarded Humanism with a question: "Granted that man has a spirit, the highest and finest expression of the possibilities that lie hidden in dead matter and waiting to be called forth by life; granted that it is worthy of our admiration and respect, and that around it can be built, as humanism tries to do, a faith in man and a reverence for what he may become that even ministers to many as a religion; granted all this, what relation can there be between this spirit and a greater one in the universe outside" (*5*, p. 160)? This facile evasion of the arguments for a humanistic thesis attests to an unmistakably supernaturalistic bias.

Illogical conclusions follow naturally: "If mind can be brought back from the limbo of unreal and imaginary things and established as something that permeates not only man's life but all life, then these other immaterial parts of him—his will, his soul, his spirit, and the rest—must also have a biological basis. . . . This is a conclusion of the utmost moment and points at the very heart of religion, for the core of a truly religious philosophy is its concern with immaterial things, with spiritual values" (*5*, p. 123).

And more, "If man's spirit is always expressed in human personalities why may we not expect the greater Spirit, as well, to be manifest as a Person? If this spirit is limitless creative power, it can provide an inexhaustible reservoir of help and strength from which man can draw freely at his need. And if man's spirit is a part of that eternal Spirit in the universe, death may not exercise dominion over it. Just as it drew dead matter together to form the living body, so it may quit the body again and return to that unseen bourn from whence it came" (*5*, p. 165). As Grant (*33*) stated in his review of the book, "the last quotation (typical of the logic used in critical places) begins with a case of reasoning-by-analogy, is followed by a mixture of three unsupported assumptions and three non-sequiturs, and then ends with another example of proof-by-analogy."

Modern Perspective

The views of scientists as a group, in contradistinction to those of Bergson, DuNoüy, and Sinnott, have not captured the imagination of a great number of readers. Partly this may be the result of the usually drab presentation employed by scientists, perhaps because in their training they were taught to avoid using language with emotional impact. Furthermore, the necessarily tentative nature of conclusions drawn by scientists is unpalatable to those reared in a cultural milieu governed by dogma which claims to be absolute, and which is acknowledged to be so by many persons in responsible positions in their communities. To advocate any doctrine derived solely from speculation as an inflexible guide to thought and behavior is directly opposed to any scientific attitude. An abstract, scholarly admonition is unlikely to replace the devil as a symbol of hell-fire and damnation in any contest of persuasion among the masses of people.

Certainly, the "ivory-tower" complex has had an important influence in keeping scholars somewhat detached from movements aimed at social reform and adult education. Too many scientists, in the past, have assumed, and many probably still do, that their obligation to society ceases with the discovery of fact; too few of them are concerned with creating a public understanding of the difference between myth and fact, or between dogma and warranted assertion. Thus the present crisis in Western culture is accentuated by a twofold "cul-

tural lag," the failure of people generally to keep pace with science in their daily life, and the failure of scientists to integrate science into the life of their communities. As in any educational situation, progress necessitates not only a social context but also a dedicated professional staff and a motivated group of students.

To be sure, a great majority of scientists are united in their opposition to the introduction of vitalism, finalism, or teleology into scientific literature. But this unity is not apparent to laymen generally. As soon as we attempt to collate the constructive opinions of scientists about the mechanistic-vitalistic controversy, there is a plethora of alternatives, even among the few opinions available in print. The gamut ranges from Russell's outright dismissal of the entire controversy as "dead," (*34*, p. 1) to the conciliatory surmise of Fuller and Tippo that, "Though it may seem contradictory, it is possible for biologists to appreciate the significance of both philosophies" (*35*, p. 23).

I suspect, however, as Mainx decided, that "the majority of experts take . . . a rather detached attitude toward them" (*25*, p. 628), and that, as Alexander wrote, "The majority of biologists adopt a view neither strictly mechanistic or vitalistic" (*7*, p. 22). Mainx's verdict seems decisive: "Both systems seek for an explanation of the organic event by reducing it to a 'principle' or a 'category' which is 'at work' in it, which 'causes' it. The concepts of 'principle' or 'category', etc., are reified into working 'causes.' It is of secondary importance whether the mechanist locates these causes in indefinite hypothetical 'elementary vital units' or in the fundamental properties of 'matter' or whether the vitalist seeks them in an 'entelechy'" (*25*, p. 628).

I do not doubt that there still may be dogmatic scientists who would assert a belief in a rigidly determined, mechanistic universe, for scientists, like any social caste or class, include individuals whose opinions reflect their cultural backgrounds as well as their hereditary potentialities or limitations, or both.

In a more constructive mien, Alexander suggested that a majority of biologists "use the methods of physical science but recognize that an organism is more than the sum of its parts. One can't explain how a whole organism works merely by analyzing separately the activities of its parts" (*7*, p. 22). Fuller and Tippo maintained that, "So long as our scientific methods are based upon objective experimentation and upon adherence to the law of cause and effect, it is logical that, whenever it is possible, our investigation should be based upon the mechanistic philosophy. However, we should be cognizant at all times that some of the phenomena with which we deal may be of a different type from those readily amenable to objective physico-chemical explanations" (*35*, p. 23).

Simpson explained: "It is compatible with materialism to hold that life and the universe involve more than objective materials and mechanism. At one end of inquiry the origin and nature of the existing materials and mechanisms have no evident materialistic explanation even though, given these as existing, their operations may be purely materialistic. At the other end the possibility is not excluded that, within the unique organization of matter that is life, these operations may develop choice, values, and moral judgment. The existence of these qualities is also a basic tenet of most vitalistic theories, which tend, however, to maintain that they are inherent in, rather than developed by life" (*19*, p. 27).

Adequate understanding involves an integration of the results of analysis and synthesis, of induction and deduction, of hypothesis and controlled experimentation, of inspiration, patience, and common sense. Unlike atoms and some molecules in gaseous or liquid form, knowledge does not diffuse autonomously; nor do I believe that it radiates from some "reservoir" of "limitless creative power." Man creates knowledge just as truly as he does machines. But, even if we acknowledge the reality of nonmaterial events and, thus, apparently, abandon "rigid" or "pure determinism," the strict necessity of empirical methodology must be emphasized explicitly, as stressed by Frank (*3*, p. 131): "The most successful scientific investigation has generally involved treating phenomena as if they were purely materialistic, and rejecting any metaphysical hypotheses as long as a physical hypothesis seems possible. The method works."

But, to be realistic, the method to which Frank refers is most effective with inanimate matter, less effective with living matter, and least effective when treating events in which human purposes and motivation are of primary concern. So it is that further improvisation is necessary in applying that method to biological and social problems. Such improvisation must be based on inspiration and ingenuity. For instance, a very stimulating discussion of what a modern mechanistic view can involve is that by Bronowski:

"A machine in science is a concept with definite properties which can be predicted. And we do not mean by this that its behavior is determined in every particular. . . . There is nothing in our concept of a machine to exclude from it a choice by tossing a penny or looking up a table of random numbers, or forecasting the future in a form which says that tomorrow will be fine three times out of ten.

"The postulate of the machine is that from the same beginnings will follow the same ends. . . . The key to the action of living things then is this, that it is directed toward the future. . . . We need see no master mind behind this and no driving purpose. It is, I repeat, the condition of life for the individual and the species. . . . The idea of a machine which is a predictor is altogether new. But it is of outstanding importance, and we must become used to it. It covers all the basic actions of living things, from the search for food in the lowest cell to the boldest creations of the human imagination. . . . A predictor is a machine which uses information about the past and present in order to make ready for the future. . . . The present is not like the future, but it is not unlike it either; it is a signal of the future; and living things, singly or in species, are predictors which interpret the signal so that they make ready for the future" (*36*, pp. 106–108).

If we hark back to Burtt's classification of modes of explanation, Bronowski's views would have to be classified as evolutionary. However, from a scientific viewpoint, evolutionary explanation with an empirical base differs radically from one with only a metaphysical base. The former can be tested experimentally, but the latter type usually cannot, and if not, then it has no significance for scientists.

However, the masses of the world's population are unaware of, or perhaps do not understand, the profound developments in scientific philosophy during the last few decades, for much of that population and even a few scientists still retain views about the nature of protoplasm and its origin which are animistic or supernaturalistic. Even in the United States, according to Riddle, "Most youth of 1954, like those of 1859, leave our schools without having an opportunity to learn that the worthy facts concerning man's origin and destiny come not from the religious traditions but from investigations made in biological and other sciences within the time of men now living" (*15*, p. 195).

The controversy of vitalism and mechanism, for example, may be "dead" for a restricted circle of thinkers, but it is very much "alive" for a great majority of religionists and for many uncommitted, and sometimes well-educated, persons as well as for a few "schizophrenic" scientists. This intellectual battle rages on.

A great deal of artificiality, nonsensical prose, and many arguments which kindle emotional overtones have their origin in static, dualistic interpretations of existence. These dualisms, such as body and mind, nature and nurture, or vitalism and mechanism, seem like dilemmas because they stifle inquiry if they are conceived as the only available choices. All of them lose significance if the pertinent facts are explained in terms of dynamic concepts as components of processes. These processes evolve under the influence of events which are integral to the universe and are not directed by principles of an absolute nature or of external sources.

But whether these dualisms refer to the nature of science, man, protoplasm, or experience is not as important as their "schizophrenic" influence upon mentation—an influence which seems to be contagious when it has a foothold in any aspect of human behavior. "There is a type of philosophic thinker," wrote Otto, "who cannot abide a world in the making. He would have everything nailed in its place in an all embracing system. But the nails he drives are conceptual nails. His hammer is clever dialectic. And what he fastens down is not the world and not the venture of life. That world and that life go rolling along, majestically indifferent to intellectual system-makers and their systems. What he fastens down is only his own thinking, his feelings as a man, and his ambition to make the best use of his talents" (*2*, p. 21).

"As a first step," admonished Otto, "we are to recognize science for what it is, a technique of inquiry, a transaction, 'in which human beings partake together with non-human things' (Dewey). We are not to be imposed upon by our use of the word, science, as a noun, so that we believe we are talking about an entity" (*2*, p. 15).

A penetrating review of some further implications of this view is that by Frank (*3*, p. 15): "We should try to think of ourselves as one of the many organisms on this vast stage of nature. . . . We are involved in complicated interrelationships and we actively participate in and help to maintain the totality we call nature. Indeed we should think of ourselves as being used by other organisms and natural processes; one of the many configurations of energy that make up the totality of existence, partaking in and being carried along by these larger equilibrating and compensating processes of nature."

"A process," continued Frank, "can never be observed directly; it can be inferred from its products and its relationships to other processes. It is a dynamic operation which can occur in a wide variety of locations and can utilize a wide variety of substances with which to produce sometimes different but basically similar or equivalent products" (*3*, p. 17). But this concept of process is practically the same as what Bronowski described as a "machine" in science. Frank and Bronowski, I believe, have essentially similar views of the nature of science, but because of their different language, they can be misinterpreted as in disagreement.

A justifiable conclusion seems to be that the vitalistic-mechanistic controversy, in its original formulation, is now almost meaningless for scientists. However, mechanism has proved to be a fruitful hypothesis in research, and for that reason the label has been conserved for modern ideas which have little resemblance to the original "mechanism." Vitalism, on the other hand, under the most elaborate pseudonyms, has tended to sterilize research as well as the thinking of its ardent devotees. As Frank concluded, "Much of the customary denunciation of science has become meaningless and irrelevant since science today is no longer mechanistic or materialistic but is dynamic" (*3*, p. 17).

Like all weapons and tools, words and language can be used harmfully, beneficially, or simply to pass the time of day. To serve adequately as a means of communication, language must be recognized as a component of human behavior and as a part of a process. When abstracted from that process and hypostatized, language loses much of its catalytic power in the learning process.

The biological nature of man and of all protoplasm, as the result of heredity and variation in an evolutionary process, must be made known universally so as to destroy the superstitious illusions and supernaturalistic dogmas of the religionisms which now threaten to destroy the social influence of science and thereby retain their sovereignty in the realm of "values" even at the cost of human existence.

In a democratic society, the unique value of which I affirm, our only means of combating ignorance is voluntary education. The vital role of education in a technical and technologic society is as yet but dimly appreciated among large numbers of the population. Its effectiveness depends on the skillful use of thought and language in converting theoretical findings into practice. One of the important needs is to create, in the minds of teachers, students, and the population generally, concepts which are as dynamic and adaptable as the processes in which people exist. Thinking and living ought not to be regimented, but they must be fettered by testable standards if all of human expectations are to approach fulfillment. That much we must learn from the history of science (*37*).

References and Notes

1. C. Darwin, *The Origin of Species* (Book League of America, New York, 1927), p. 499.
2. M. Otto, *In Honor of John Dewey on his Ninetieth Birthday* (Univ. of Wisconsin Press, Madison, 1951), p. 17.
3. L. K. Frank, *Nature and Human Nature* (Rutgers Univ. Press, New Brunswick, N.J., 1951).
4. M. Otto, *The Human Enterprise* (Crofts, New York, 1947), p. 297.
5. E. W. Sinnott, *The Biology of the Spirit* (Viking, New York, 1955).
6. C. Singer, *A History of Biology* (Schumann, New York, 1950), p. 355.
7. G. Alexander, *General Biology* (Crowell, New York, 1956).
8. C. Villee, *Biology* (Saunders, Philadelphia, Pa., ed. 2, 1954), p. 10.
9. T. Dobzhansky, *Evolution, Genetics and Man* (Wiley, New York, 1956).
10. E. Radl, *The History of Biological Theories* (Oxford Univ. Press, London, 1930), p. 352.
11. A. Mitchell, *Bergson's Creative Evolution* (Holt, New York, 1931), p. 50.
12. M. White, *The Age of Analysis* (New American Library, New York, 1955), p. 67.
13. J. E. Creighton, "Bergsonism," in *Encyclopedia Americana* (Encyclopedia Americana, Chicago, Ill., 1949), vol. 3, p. 548.
14. H. W. Smith, *Man and His Gods* (Little, Brown, Boston, Mass., 1955).
15. O. Riddle, *The Unleashing of Evolutionary Thought* (Vantage, New York, 1955).
16. P. Sears, review of *The Unleashing of Evolutionary Thought*, *Sci. Monthly* **82**, 317 (1956).
17. L. DuNoüy, *The Road to Reason* (Longmans Green, New York, 1949), p. 189.
18. C. Mayer, *Man: Mind or Matter?* (Beacon, Boston, Mass., 1951).
19. G. G. Simpson, *The Meaning of Evolution* (Yale Univ. Press, New Haven, Conn., 1949).
20. E. A. Burtt, *The Metaphysical Foundations of Modern Science* (Doubleday, Garden City, N.Y., 1954).
21. H. H. Newman, *Evolution, Genetics and Eugenics* (Univ. of Chicago Press, Chicago, Ill., 1921), p. 13.
22. L. DuNoüy, *Human Destiny* (Longmans Green, New York, 1947).
23. M. F. Guyer, *Animal Biology* (Harper, New York, 1937), p. 22.
24. H. Reichenbach, *The Rise of Scientific Philosophy* (Univ. of California Press, Berkeley, 1951).
25. F. Mainx, "Foundations of biology," in *International Encyclopedia of Unified Science* (Univ. of Chicago Press, Chicago, Ill., 1955).
26. S. J. Holmes, *An Introduction to General Biology* (Harcourt, Brace, New York, 1926).
27. W. LaBarre, *The Human Animal* (Univ. of Chicago Press, Chicago, Ill., 1954).
28. D. W. Gottschalk, *Structure and Reality* (Dial, New York, 1952).
29. H. F. Blum, *Time's Arrow and Evolution* (Princeton Univ. Press, Princeton, N.J., 1951).
30. A. I. Oparin, *The Origin of Life*, translated by S. Morgulis (Dover, New York, 1953).
31. G. Wald, *Sci. American* **191**, 44 (1954).
32. M. Calvin, *Am. Scientist* **44**, 248 (1956).
33. M. L. Grant, *Humanist* **1956**, 193 (1956).
34. E. S. Russell, *The Directiveness of Organic Activities* (Cambridge Univ. Press, Cambridge, England, 1945).
35. H. J. Fuller and O. Tippo, *College Botany* (Holt, New York, 1954).
36. J. Bronowski, *The Common Sense of Science* (Harvard Univ. Press, Cambridge, Mass., 1955).

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How Adequate Is the World's Food Supply?

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THE size of the task of producing and distributing, every day, enough food to feed approximately 2652 million persons (1954 statistics) can, perhaps, be visualized in terms of the family dinner table. Imagine a table set up around the world on the equator, with 2 feet of space allowed per person on each side. To feed all the people at one sitting would require 20 such tables, together with a small auxiliary table 4232 miles long. In addition, a sizable crew of carpenters would be kept busy building more tables. At the rate at which the population was increasing annually during the period 1950–54 (1.3 percent), the carpenters would have to construct a little more than 17.8 miles of table every day. Thus, by 1960, 21 tables stretching around the earth at its center, plus an auxiliary table about 18,500 miles long, would be required.

Putting food on the table for well over 2.6 billion people is no small task. It is not merely the task of the housewife who prepares the food, nor is it the sole concern of the nutritionist who studies whether or not the calories and vitamins are sufficient, whether the protein is adequate in quantity and quality, and whether the minerals are present in sufficient quantities and in proper balance. Putting food on the world's tables requires the attention of all those who are concerned with food production, processing, distribution, and consumption.

While recognizing that this task of feeding the people of the world is a gigantic one, it is equally important for us to recognize that the nature of the problem differs in the various portions of the globe. There are substantial differences in the quantities of food consumed per person in different regions and also differences in the quality of that food.

Variations in Quantity and Quality

Many measures of quantity and quality of food consumed might be selected to show these variations. If one is to present a broad picture, it is necessary to select measures of a rather general nature and for which data are available from a reasonably representative number of countries or areas. For the present purpose, total calorie intake has been selected as a measure of quantity, while intake of total protein and animal protein have been selected as measures of quality of diet.

Man derives the calories he needs from all types of food, although foods that are rich in carbohydrates or fats, or both, are usually regarded as the main sources of energy. The carbohydrate-rich foods such as cereals and potatoes are usually among the least expensive sources of energy. Such foods provide much of the energy required both for the maintenance of normal body metabolism at rest and for the performance of work. In addition, man must have protein, both for the building of muscle tissues and for the repair of these tissues. Proteins vary in their value, depending on the types of amino acids of which they are composed. Man obtains his protein from both plant and animal sources, and that from animal sources is generally the more adequate in amino-acid content in relation to man's needs, although there are exceptions to this general rule. Even though no general agreement has been reached on minimum and optimum allowances of total protein and animal protein, an indication of optimum requirements is found in Morris' (*1*) suggestion of 70 grams of total protein and 33 grams of animal protein per day, these levels being based on recommendations of the National Research Council (U.S.A.).

Calorie requirements differ in different parts of the world. The League of Nations (*2*) published estimates of food requirements, according to which, for example, an adult male performing 8 hours of moderate work would require 3000 calories per day. In the United States, the National Research Council (*3*) published scales of calorie requirements which indicated 3000 calories per day for a physically active man. Among the few scales of calorie requirements published for populations other than those of the Western nations are the scales suggested by the League of Nations (*4*) Inter-Governmental Conference on Rural Hygiene, according to which the daily requirement of an average man in India is 2600 calories and in Japan, 2400 calories.

Data for 33 countries or areas, drawn from the Food and Agriculture Organization's *The State of Food and Agriculture—1956* (*5*), are presented in graphic form in Figs. 1 and 2. The data are not all for the same year, since a choice had to be made between having complete comparability of years and having a larger sample. Also, the accuracy of the data on which these graphs are based naturally varies from country to country, depending on the degree to which each country's statistical service is developed. However, in spite of certain deficiencies, these figures and others used in this article are from the most dependable sources available, and they bring out in a striking manner the wide variations which now exist in food consumption.

Variations in calorie content of available food supplies (Fig. 1) in these 33 countries or areas ranged from 3545 per person per day in the Republic of Ireland to 1840 in India. Thus, the average person in Ireland had available nearly twice as many calories as did his counterpart in India.

Variations in total protein content (Fig. 2) ranged from 99 grams per person per day in New Zealand to 50 in India. Thus, as was the case with calories, the average person in the country having the largest amount of protein available per person had approximately twice as much protein as did the average person in the country with the smallest amount.

Variation in availability of animal protein is much more striking than that for calories and total protein, as may be seen from inspection of the graph for animal protein in Fig. 2. Uruguay ranked highest in availability of animal protein, with 67 grams per person per day, while India had only 6 grams per person. Thus, the highest ranking country exceeded the lowest by a little more than 11 times. As will be seen from the two curves, the countries having the lower amounts of total protein are also, in many cases, those having very low amounts of animal protein.

Data of a similar nature (*5*), showing variations between regions and the trends from the pre-World War II (1934–38) period to 1953–54, appear in Figs. 3, 4, and 5 for calorie intake, total protein intake, and animal protein intake, respectively. Graphs showing average intake per person per day are presented for five regions—Oceania (Australia and New Zealand), North America (Canada and the United States), western Europe, Latin America, and the Far East, and for

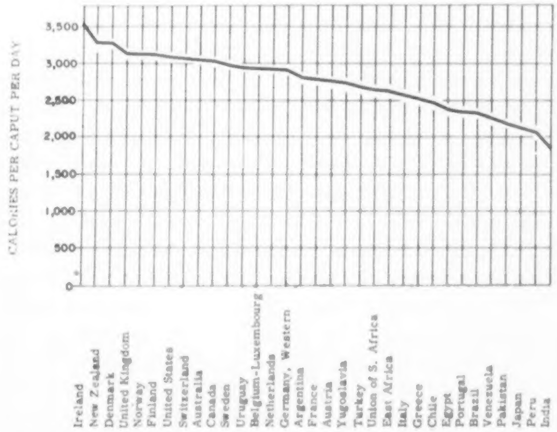

Fig. 1. Variations in estimated number of calories available per person per day in food supplies of 33 countries or areas. Data are for 1953–54, except for Latin American countries, where data are for 1952 for Argentina, Brazil, Chile, Peru, and Uruguay and for 1951 for Venezuela.

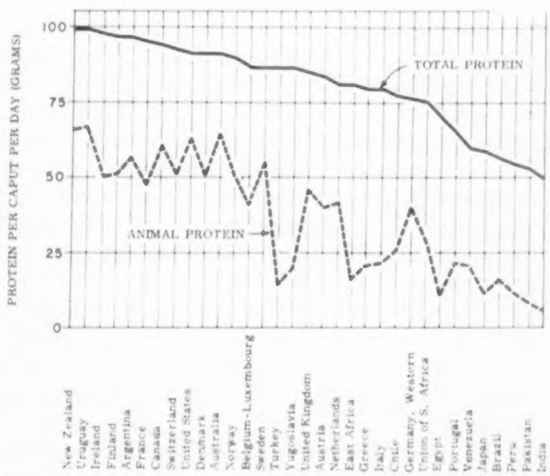

Fig. 2. Variations in estimated number of grams of total protein and animal protein available per person per day in food supplies of 33 countries or areas. Data are for same years as those in Fig. 1.

two separate countries—Turkey and Egypt—in the Near East area. These graphs reveal, in a striking manner, the wide variations in quantity and quality of diet which exist between regions, just as the variations between countries are shown in Figs. 1 and 2. Here, also, the most striking differences are found in the intake of animal protein.

Economic Development and Food Supplies

Study of these charts naturally leads one to a comparison of the data in the "developed" and "underdeveloped" countries and regions. These terms *developed* and *underdeveloped* are relatively new in our vocabulary. The division can be regarded only as a broad indication of the relative states of development, for exceptions in detail can be found in every region as well as in every country. For example, the United States is regarded as having a highly developed agriculture. Yet, anyone who knows the various parts of the country can easily indicate areas where agriculture is underdeveloped. On the other hand, Yemen, for example, is a country which has remained isolated from modern technical and economic developments, yet it has some of the best terracing in the world. So, while its agriculture is, in general, extremely underdeveloped, it is well developed in this one aspect of soil conservation. It is, of course, very difficult to distinguish between the level of agricultural development and that of economic development generally.

Among the "developed" countries, those usually included are the countries of northwestern Europe, Oceania, and North America, while the countries of southeastern Europe and of the Near East and most of the countries in Africa, Asia, the Far East, and Latin America are usually regarded as being underdeveloped. A rough indication of the relationship which now exists between state of development and quantity and quality of diet is found in the broad comparison of regions in Figs. 3, 4, and 5. This relationship also becomes apparent if one examines the relative positions of the countries for which data are shown graphically in Figs. 1 and 2 for availability of calories, total protein, and animal protein. If one tabulates the countries or areas falling in the first, second, and third portions of each sequence according to whether each country or area is generally considered to be developed or underdeveloped, the results shown in Table 1 are obtained.

The two countries usually regarded as being underdeveloped which appear in the highest one-third in Table 1 for both protein and animal protein content of the diet are Uruguay and Argentina. Both are well-known producers of livestock, and

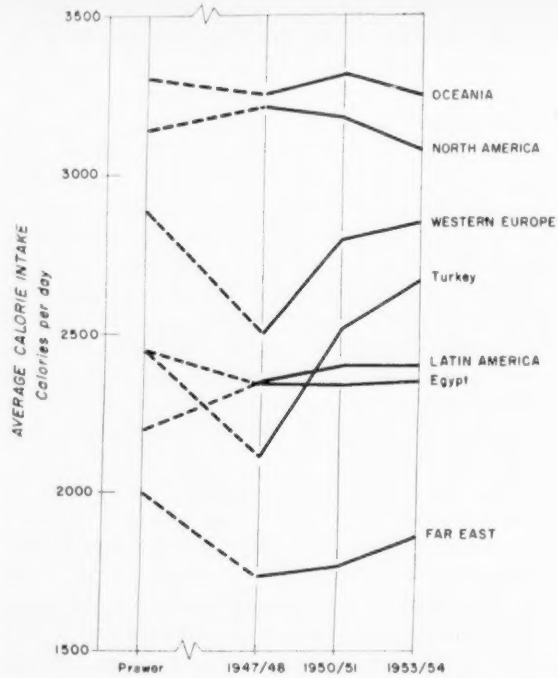

Fig. 3. Average calorie intake, in calories per person per day, for five regions and two countries: pre-World War II (1934–38), 1947–48, 1950–51, and 1953–54. The five regional groups include countries as follows: Oceania (Australia and New Zealand); North America (Canada and the United States); western Europe (Austria, Belgium, Luxembourg, Denmark, Finland, France, West Germany, Greece, Ireland, Italy, the Netherlands, Norway, Portugal, Sweden, Switzerland, United Kingdom, and Yugoslavia); Latin America (Argentina, Brazil, Chile, Colombia, Mexico, Peru, Uruguay, and Venezuela); Far East (Ceylon, India, Japan, Pakistan, and the Philippines).

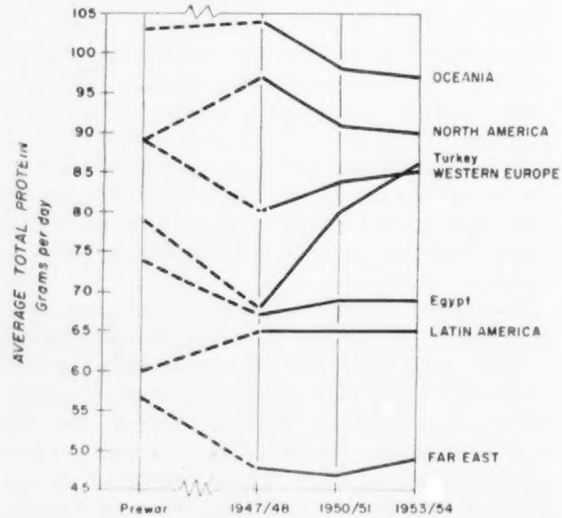

Fig. 4. Average total protein intake in grams per person per day.

both have long-standing traditions of high meat consumption.

Consumption of Cereals and Meat

Another indication of the wide variation in the content of the diet in different countries may be had by comparing the proportions of the total number of calories which come from cereals. Data for 16 countries, showing trends from the pre-World War II period of 1934–38 to 1953–54 (*6*) are shown in graphic form in Fig. 6. People in countries such as Egypt, India, and Japan obtain approximately 65 to 70 percent of their total calories from cereals, while those in Italy and Greece are slightly less dependent on cereals, the proportion of calories obtained from this source being between 55 and 60 percent in 1953–54. The figures for 11 other countries included in this comparison ranged from approximately 22 to 40 percent in 1953–54, the people of Canada and the United States obtaining the smallest proportions of their calories from cereals, followed closely by the people of New Zealand and Australia.

Meat consumption (beef, veal, pig meat, mutton, lamb, and goat meat) provides an equally interesting, and even more markedly contrasting, indication of the content of the diet, as may be seen in Fig. 7. Data for 32 countries are summarized in graphic form, including data for the 16 countries for which data are given in Fig. 6 on proportion of calories obtained from cereals.

Near the end of World War II, and in the years just after that war, there were some who claimed that the United States must gradually reduce its meat consumption and depend to a greater degree on cereals and on other foods which could be consumed directly without the loss of energy which is inevitable when animals turn plant products into meat. If these claims should prove to be correct, owing to substantially increased population pressures or other factors, it could mean a marked change in the eating habits of the average American. To put the problem in an extreme form, try to plan the menu of your family for a week on the assumption that 70 percent of the total number of calories is to be obtained from cereals and potatoes and that somewhat less than 0.5 pound of meat per person per week will be served. An examination of Figs. 6 and 7 will reveal that this theoretical situation is roughly comparable to the average situation in Egypt in 1953–54.

Production Trends and Food Requirements

In addition to the comparisons made thus far, which bring out clearly some of the variations in quantity and quality of diet among countries and regions, it may be of interest to examine recent production trends in the various regions in relation to population trends.

The rise in world agricultural production, only slight in 1954–55, was rather more sharp in 1955–56, and the average annual increase over the last 5 years of 2.6 percent is about 1 percent ahead of the annual growth of world population.

The main increase in 1955–56, however, was in the regions most troubled by surpluses. Production rose considerably in North America for the first time since 1952–53, despite acreage limitations in

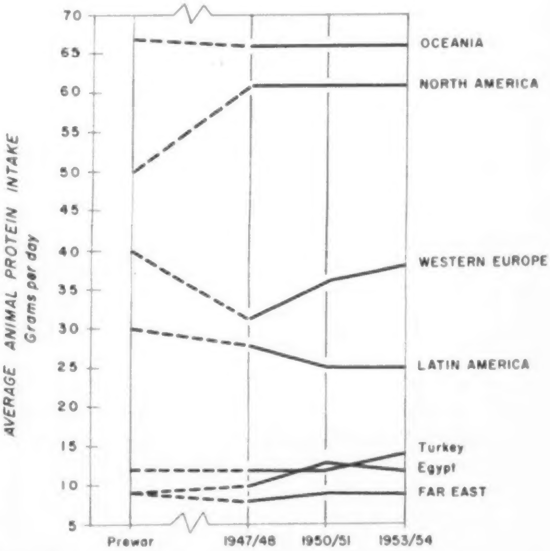

Fig. 5. Average animal protein intake in grams per person per day.

Table 1. Relation of quantity and quality of diet to the state of development of countries or areas, based on relative status of the countries or areas for which data are shown in graphic form in Figs. 1 and 2.

Relative status of countries or areas	Number of countries or areas	
	Developed	Under-developed
	Calorie content	
Highest one-third	11	0
Middle one-third	6	5
Lowest one-third	0	11
	Protein content	
Highest one-third	9	2
Middle one-third	6	5
Lowest one-third	2	9
	Animal protein content	
Highest one-third	9	2
Middle one-third	8	3
Lowest one-third	0	11

the United States, and there was also a marked rise in Oceania. In the rest of the world the increases were smaller, although the Far East registered its largest gain for some years, and in some regions production declined slightly. Production developments in 1955–56 thus did little to reduce the long-standing inequalities between the well-fed and the underfed regions (Figs. 8, 9).

It is hardly possible, within the limits of a brief article, to deal adequately with all the important reasons for variations in the food supply. Therefore, brief mention will be made of only a few of the basic reasons. These include variations in the productivity of the land, variations in population pressure on the land, variations in food requirements, and differences in the level of economic development.

In his efforts to find a livelihood, man has now settled in most of the readily accessible and productive portions of the globe as well as in many portions which cannot be described as being either readily accessible or highly productive. Therefore, in considering the sources of food, it is worth while to recall just how little of the earth's surface may be classified as suitable for agricultural production.

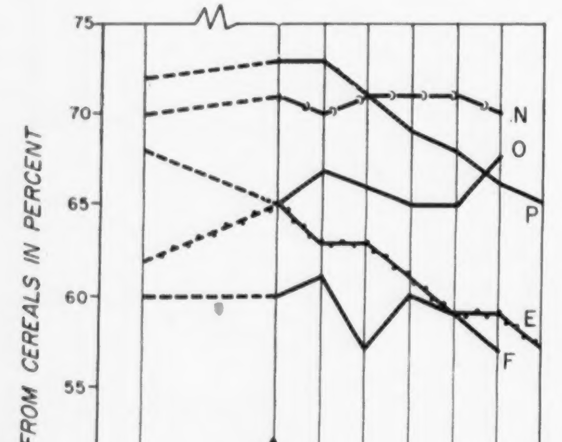

Fig. 6. Proportion of total calories from cereals, in 16 countries, showing trends from pre-World War II through 1953–54. (*A*) Australia, (*B*) New Zealand, (*C*) United States, (*D*) Canada, (*E*) Italy, (*F*) Greece, (*G*) Austria, (*H*) France, (*I*) West Germany, (*J*) Norway, (*K*) the Netherlands, (*L*) United Kingdom, (*M*) Denmark, (*N*) Egypt, (*O*) India, (*P*) Japan. [Adapted from *The State of Food and Agriculture—1955*]

Capacity of the Earth to Produce Food

The earth's surface is divided between water and land approximately as follows: water—141,050,000 square miles; land—55,786,000 square miles. Thus, 71.7 percent of the earth's surface is covered by water, and only 28.3 percent is land.

The oceans, lakes, and rivers are important sources of food. However, the total production of food and other products from these sources is small when compared with products from the farming and grazing areas of the world. Rough estimates, based on the most reliable statistics available, indicate that the value of the world's production from agricultural and forested lands and from its water areas is divided roughly as follows: agricultural production—87.5 percent; forestry production—11.0 percent; fisheries production—1.5 percent.

About one-fifth of the land surface is in the permafrost areas of Antarctica, Greenland, Canada, Alaska, and the U.S.S.R. Also, according to Stamp (*7*), more than one-fifth of the land surface is either too rugged or too lofty to permit permanent settlement that is dependent on cultivation. Stamp also estimates that another fifth of the land surface is too arid for cultivation and cannot be irrigated by methods of watering now available. Thus, the habitable lands, having physical and climatic conditions that theoretically permit the growth of crops, constitute no more than two-fifths of the land surface. Moreover, much of this area is not inhabited or is only sparsely populated. The Amazon basin, for example, occupies some 2 million square miles, according to Stamp, and while it has a great deal of natural vegetation, it supports only a very limited population.

Fawcett (*8*) came to the conclusion that only about 30 percent of the total land area was cultivable or arable. This includes the wet, forested lands of the tropics which seem to offer the main possi-

bilities for extending the area of actual cultivation, although important technical problems remain to be solved before such cultivation can be undertaken on a large scale. Fawcett estimated that another 30 percent, including forests and grazing lands, might be classed as productive but not arable. Applying these percentages to the world's land area, one arrives at the figures shown in Table 2. The arable land, about 10,710 million acres, amounted to slightly more than 4 acres for each of the 2652 million people living in 1954. However, according to Stamp (7), the land actually cultivated is only between 3000 and 4000 million acres, or between 1.1 and 1.5 acres per person, based on the world's population in 1954.

These are, admittedly, rough approximations, but they are sufficiently accurate to give a good indication of the limitations placed on food production by the amount of arable land available.

A further figure of 1352 million hectares (about 5,220,000 square miles or 3341.8 million acres) is found in the latest estimate of arable land and land under tree crops (not forests) published by the Food and Agriculture Organization (9). This is about 2.65 percent of the total surface of the earth and amounted to about 1.25 acres per person in 1954. These figures do not include Tibet and some minor areas, for which no substantive information is available.

Within the arable areas there are marked variations in levels of production. For example, during the period 1945–47, wheat production varied from 43.8 bushels per acre in Denmark to 8.8 in India. In the same period, rice production varied from 51.3 bushels in Australia and 47.5 bushels in Spain to 12.4 in the Philippines. Corn or maize varied from 31.0 bushels in the United States to 9.6 in Bulgaria and India and 9.4 in the island of Java. These three examples from among the world's most important food crops indicate something of the extent of variations in productivity which result from differences in temperature, water supply,

Table 2. Food-producing capacity of the land area of the earth.

Type of surface	Square miles	Percentage of land area	Percentage of total area
Arable land	16,735,800	30	8.5
Forest and grazing land	16,735,800	30	8.5
Unproductive land	22,314,400	40	11.3
Total	55,786,000	100	28.3

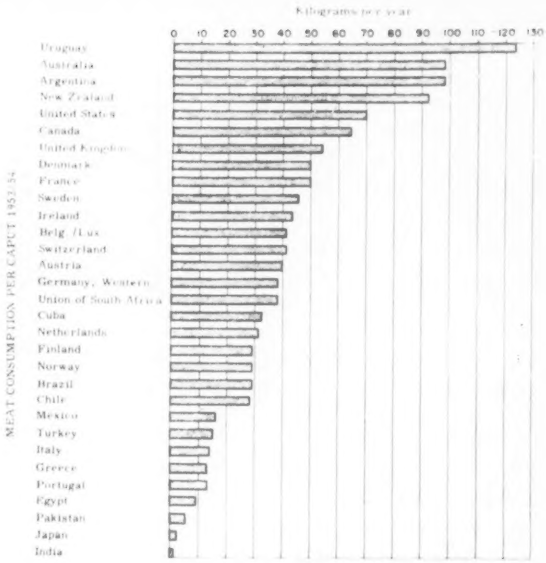

Fig. 7. Meat consumption (beef, veal, pig meat, mutton, lamb, and goat meat) in 32 countries (including the 16 for which data on calories from cereals are shown in Fig. 6) in 1953–54, in kilograms per person per year. [Adapted from *The State of Food and Agriculture—1955*]

natural fertility of the soil, use of fertilizers, tillage practices, and other factors. They are based on Stamp's (7) tabulations, extracted from statistics published by the Food and Agriculture Organization.

Some of the variations in the productivity of the land surface of the earth are illustrated in Figs. 10–14, which show types of land surface that range from the grazing land of the Tibetan highlands to the intricately terraced rice fields on the island of Java in Indonesia.

Population Pressures and Land Productivity

Population pressures and the rate at which they are increasing are discussed briefly at the beginning of this article. The extent to which they vary in relation to land area was brought out by Fawcett (*10*) in his article on the numbers and distribution of mankind. He pointed out that, in what he called the four major human regions, the population per square mile was 400 in India, 292 in the Far East, 186 in Europe, and 52 in eastern North America. Such variations naturally have a marked effect on the extent to which a population can produce food enough to meet its needs.

The outcome of the "race" between populations and food supplies has been the subject of many papers since the Reverend Thomas Robert Malthus

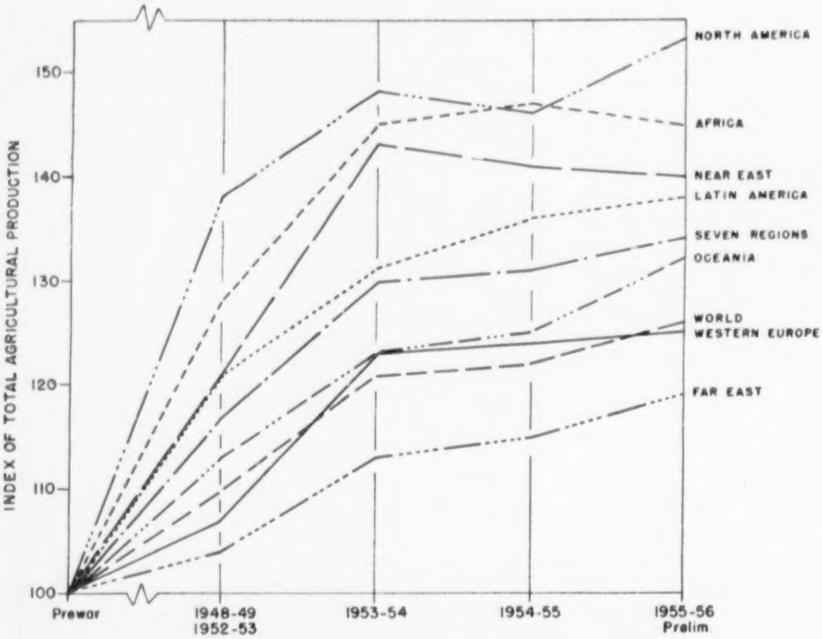

Fig. 8. Changes in the volume of total agricultural production, pre-World War II through 1955–56, in seven regions (individually and averaged) and in the world as a whole (including estimates for the U.S.S.R., eastern Europe, and China). Pre-World War II average equals 100 in each case. [Data from *The State of Food and Agriculture—1956*]

called attention to the problem. Malthus (*11*), in his *Essay on the Principles of Population,* which was first published in 1798, put forth the idea that populations increase more rapidly than do food supplies. Therefore, so Malthus argued, there would always be more people in the world than could be fed, and wars and disease would be necessary to dispose of the surplus population. This idea became known as the Malthusian theory, although Malthus based his argument on the works of other economists, including Adam Smith.

An ardent supporter of the Malthusian theory might initiate a discussion on the world's food supply by examining the situation in a country or countries where the population has outrun the food supply or is in danger of outrunning it. He would no doubt speak of the teeming millions in southern and eastern Asia, of the small farms, of the low protein diet based on rice, and of the generally low economic level of most of the people. He would also speak of the problems of producing enough food in the Near East, where in many areas life depends on irrigation water, where the desert may begin where irrigation canals end (as it does in Egypt), and where the average peasant lives under very poor conditions indeed.

An opponent of the Malthusian theory, more optimistic regarding man's future, would no doubt take a different point of departure. He might speak of the wide varieties of excellent food available in American supermarkets, of the productivity of the "corn belt," of potential increases in production on the pampas of Argentina, and of new dams in the upper reaches of the Nile or on the great rivers of India and Pakistan before these flow out of the Himalayas. He might also speak of the possibility of developing as yet untouched tropical lands, such as those in much of the Amazon basin.

It has not been my intention, in this article, either to support or to oppose the Malthusian theory, but rather to set forth some facts about the world's food supply, particularly the variations which now exist in that food supply in different parts of the world and some of the reasons for these variations. Whether the Malthusian theory should eventually prove to be right or wrong may, in any event, depend primarily on man's wisdom in dealing with his food production and population problems rather than on the earth's capacity to produce food for a population which expands without regard to the application of some of that wisdom.

Variations in Man's Food Requirements

Major factors which affect man's calorie requirements include sex, body size, degree of physical activity, age, external temperature, pregnancy, and lactation. The effects of these and other factors on calorie requirements have been the subject of special study by the nutrition division of the Food and Agriculture Organization. The nutrition division has been assisted by two committees which pre-

pared reports (*12, 13*), the second committee having refined and extended the findings of the first.

The method proposed for arriving at the calorie requirements of populations and population groups was to assign numerical value to the calorie requirements of a fully defined "reference" individual and to indicate (also in numerical form) the adjustments to these values which must be made in order to calculate the requirements of individuals differing from the reference in age, body size, temperature of environment, and activity.

The reference man is 25 years of age. He is healthy; that is, he is free from disease and is physically fit for active work. He weighs 65 kilograms. He lives in the temperate zone at a mean annual temperature of 10°C. He consumes an adequate, well-balanced diet and neither gains nor loses weight. On each working day he is employed 8 hours in an occupation which is not sedentary but does not involve more than occasional periods of hard physical labor. When not at work, he is sedentary for about 4 hours daily and may walk for as much as 1.5 hours. He spends about 1.5 hours in active recreation and household work. The reference man, as defined, is assumed to require, on an average for the entire year, 3200 calories daily.

The reference woman is a similarly healthy person, aged 25 years and weighing 55 kilograms. She lives in the same environment as the reference man. She may be engaged either in general household duties or in light industry. Her daily activities include about 1 hour of walking and also 1 hour of active recreation, such as gardening, playing with children, or nonstrenuous sport. The reference woman is assumed to require, on an average for the entire year, 2300 calories daily.

Various adjustments were then proposed which would take into account variations in body size, physical activity, age, external temperature, pregnancy, and lactation. Applying the findings published by the Food and Agriculture Organization (*13*) to a few countries, one finds that the average requirement per person in the United States is 2610 calories per person per day, while the requirements in Japan and India are 2310 and 2230, respectively. These three countries are cited only as examples, to show something of the variation which exists, based on differences in the size of the people, effects of external temperature, distribution of age groups in the population, and other factors.

Other Factors Affecting Food Supplies

Differences in food consumption arising from variations in the level of economic development may be attributed to many factors. These include the small size of the farms and the fragmentation of holdings which exist in many regions of the world; the lack of transport, which prevents ready marketing of products; the lack of good market outlets in cities—the result either of poor marketing facilities or of low purchasing power of the city

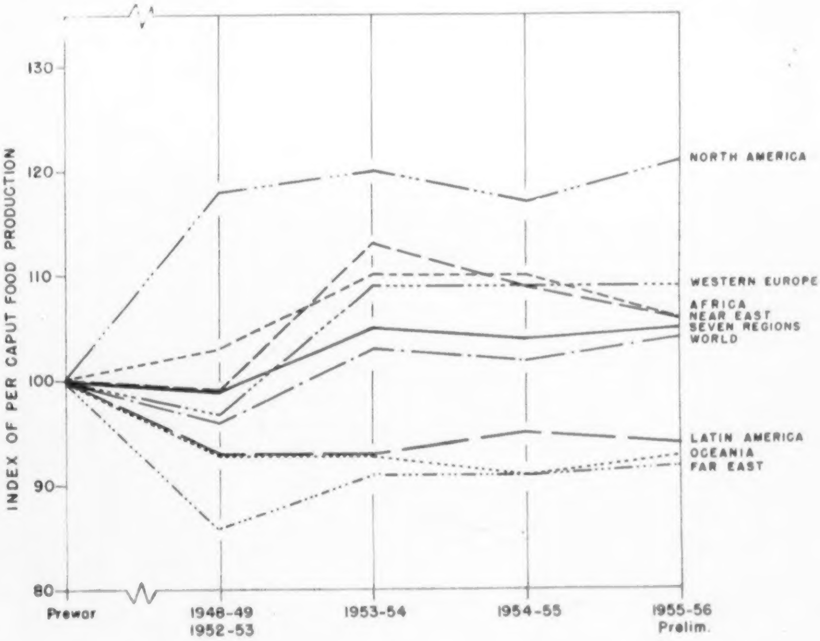

Fig. 9. Changes in the volume of food production per person, pre-World War II through 1955–56. Prewar average equals 100 in each case. [Data from *The State of Food and Agriculture—1956*]

Fig. 10. Tibetan woman milking a yak in the Tibetan highlands, at an elevation of about 10,000 feet. Yaks, sheep, and goats are grazed at this and higher elevations during the summer and are brought down to the somewhat lower valleys during the winter.

populations; the lack of credit, which prevents farmers from increasing their producing capacity through the use of fertilizers, better seeds, or other means; and inadequate facilities for preserving food during the seasons of large supply. Associated problems arise from the effects of dietary habits and religious and other customs on consumption of agricultural products, which sometimes place serious difficulties in the way of achieving well-balanced diets.

These and many other factors which come under the general heading of economic development are far too complicated to be discussed here in detail. Only one item will be mentioned, as an indication of the way in which the general level of economic development may affect the level of agricultural production.

Commercial fertilizers have been an important factor in achieving and maintaining a high level of production in countries such as the United States, Canada, and Japan and in northwestern Europe. By contrast, fertilizers have made little contribution in most of the underdeveloped portions of the world, where farmers have not learned to know their value, lack the funds with which to buy them, and, in any event, usually lack a ready source of supply. The total world consumption of actual plant nutrients (N, P_2O_5, and K_2O) contained in the fertilizers used in the "fertilizer year" 1955–56 was 19.4 million tons. Of this amount,

Fig. 11. Mountain farming area in Austria, in the valley between Innsbrück and the Brenner Pass.

16.9 million tons were used on a little more than one-third of the world's agricultural land, in western Europe, the United States, Canada, Japan, Australia, and New Zealand, while only 2.5 million tons were used on the remaining nearly two-thirds of the agricultural land, which is distributed over the greater part of Asia and the whole of Africa and over Latin America and eastern Europe (*14*).

Conclusion

Although the Malthusian theory has been left for others to debate, one can hardly conclude an article of this kind without recording some additional thoughts about the problems with which man is faced in meeting his food requirements.

The problem of finding enough food for all the world's people has been the concern, during the last three decades, not only of agriculturalists, nutritionists, economists, and politicians but, voluntarily or involuntarily, of almost every thinking man and woman. Their attention has been focused sharply on this problem by the events that led up to World War II, by the gigantic feeding problems which had to be met during and following that war, and by the recent upsurge in population numbers. Even those who did not think about the problem could hardly avoid being affected, unless they lived in some remote jungle that was untouched by world events.

Man's concern with finding enough food is not new. It occupied an important place in his thinking long before population curves rose to their present level. To find evidence of this, one has only to turn to the book of Genesis. In the description of the Creation, one finds recurring references to the earth bringing forth grass, herbs, and fruit trees;

Fig. 12. Intensive hillside cultivation on terraces near Revello, above the Amalfi coast in Italy.

Fig. 13. Fields used primarily for cereal production in a high valley near Bamian, in the Hindu Kush in Afghanistan. The figure carved in the mountain side is the world's tallest Buddha. Holes in the mountain side are entrances to caves, formerly used as homes but now largely abandoned. Agriculture in this area has changed little since Biblical times, and crop production is largely limited to the narrow valleys, where irrigation is possible.

Fig. 14. Terraced rice fields in Java, Indonesia, shortly after the rice had been transplanted, showing the extreme amount of labor utilized in some parts of the world to wrest a living from the land.

to the creation of fish, fowl, and cattle; and to man's being given dominion over these things. There is also reference, near the end of the first chapter, to seeds and fruits being "meat" for man and to the green herbs being "meat" for the beasts and fowl. It is only in the third chapter of man's history, as described in Genesis, that food of an especially attractive type—fruit of the tree of knowledge of good and evil—provided the first temptation. As the earth's population grew, and as political organization began to take shape, Nimrod, presumably because of his prowess as a hunter and, therefore, a good provider of food, became the first monarch, according to the Biblical account.

In the 20th century, man is still very much concerned with finding enough food. The main difference lies in the magnitude of the problem. Large areas may be devastated because the land is misused in man's efforts to produce food; unscrupulous political leaders, tempted not as Eve was by the fruit of a tree but by the lush fields in neighboring countries, may start conflicts or by other means undertake to grab land, affecting the lives and fortunes of millions; or large numbers of people may perish because of famines resulting from crop failures or from floods which destroy their food supplies. Leaving aside crop failures and floods, production in some parts of the world is insufficient to provide all the people with adequate food; where calories are sufficient, there may be wide variations in the quality of the diet.

In areas where there is adequate productive land available per person, where there is a good balance between crop and livestock production, where economic developments have been such as to provide good markets, and where there are research, extension, and other services to support the farmer in his effort to produce, one finds a more adequate and better balanced food supply. The present-day American is particularly fortunate in this respect.

The "supermarket," and the many other food markets which do not carry this label, with their wide variety of foodstuffs, all neatly packaged, from many different kinds of agricultural areas throughout the United States and from sources outside the country, are becoming so commonplace that many Americans are likely to forget the marvels of food production and distribution which make such food markets possible. The new generation, accepting both the modern methods of marketing and the wide variety of foods just as casually as it accepts television and as it is beginning to accept atomic energy, is likely to take for granted the bounty of a land which is unique in the history of mankind.

This does not mean that the United States has solved all its food production and distribution problems, either for the present or for the growing population of the future. It does mean that the American consumer, having ready access to foods from widely differing production areas, through an effective distribution system in a country which can still produce more food than it requires, can be well fed with less effort than his counterpart in practically any other part of the world.

For the world as a whole, many problems of production and distribution, and of economic development generally, remain to be solved before all the people of this generation or the next, or of the generation after that, can be adequately fed. Those problems have been presented here only in very broad outline, for, to quote J. H. Woodger, "the situation is complicated and its difficulties are enhanced by the impossibility of saying everything at once."

References

1. J. N. Morris, *Lancet* **248**, 743 (1945).
2. *Report on the Physiological Bases of Nutrition*, vol. II of *The Problem of Nutrition*, League of Nations Publs., ser. II (1936), p. 4.
3. *Recommended Dietary Allowances* (National Research Council, Washington, D.C., 1948).
4. *Health*, League of Nations Publs., ser. III (8 Sept. 1937), pp. 65–77.
5. *The State of Food and Agriculture—1956* (Food and Agriculture Organization of the U.N., Rome, Italy, 1955).
6. *The State of Food and Agriculture—1955* (Food and Agriculture Organization of the U.N., Rome, Italy, 1955).
7. L. D. Stamp, *Land for Tomorrow* (Indiana Univ. Press, Bloomington, Ind., 1952).
8. C. B. Fawcett, *Geographical J.* **76**, 504 (1930).
9. *Yearbook of Food and Agricultural Statistics* (Food and Agriculture Organization of the U.N., Rome, Italy, 1955), vol. ix, pt. 1.
10. C. B. Fawcett, *Sci. Monthly* **64**, 389 (1947).
11. T. R. Malthus, *An Essay on the Principles of Population* (Reeves and Turner, London, ed. 9, 1888).
12. *Calorie Requirements*, Nutritional Study No. 5 (Food and Agriculture Organization of the U.N., Washington, D.C., 1950).
13. *Calorie Requirements*, Nutritional Study No. 15 (Food and Agriculture Organization of the U.N., Rome, Italy, 1957).
14. H. J. Page, *An Annual Review of World Production and Consumption of Fertilizers—1956* (Food and Agriculture Organization of the U.N., Rome, Italy, 1956).

Interior of the Human Heart

Two photographs of the heart valve (mitral valve) that prevents backflow from the left ventricle into the left atrium as seen from the ventricle. The upper photograph shows the valve after pressure of fluid in the ventricle has forced it shut as would normally happen at the beginning of ventricular contraction or systole; the chordae tendineae (or the heart strings of literature), which prevent relapse of the valve, have been stretched taut by the pressure. The lower photograph shows the valve forced open by inrushing fluid from the atrium as would normally happen when the atrium begins its contraction and the ventricle relaxes in diastole. The chordae tendineae are relaxed by the movement of the valve toward the viewer.

To produce action of the valves, water was run through the heart under varying pressure, thus imitating the flow of blood during life. The photographs, which form a sequence in the motion picture, *Red River of Life*, were taken with a 35-mm motion picture camera which operated in synchrony with a stroboscopic flash lamp inserted in the ventricle. The work was done by G. Keith Hargett and Edwin C. Udey of the Moody Institute of Science, Los Angeles.

LETTERS

Traditional Spelling

Our traditional spelling should be abandoned; it very imperfectly fulfills the purpose for which it was intended. David Diringer, in his book *The Alphabet*, tells us that alphabetic writing has been an unsuccessful attempt to represent by symbols the sounds of the human voice. Bernard Shaw in his will donated all his money for phonetic reform.

But Noah Webster's complete success in creating a perfect phonetic written language seems to have been overlooked. He gives us two different spellings for the majority of our words—(i) the incorrect or traditional spelling and then (ii), in parentheses, the corrected or phonetic spelling. The latter, simplified spelling corresponds exactly with the spoken words.

We use the corrected spelling in pronouncing our words and the incorrect spellings in writing them. In the parentheses we have the graphic counterpart of speech. This is David Diringer's definition of perfect symbolism.

In deciphering the sounds from the traditional writings, it is necessary to weed out all the unnecessary letters, often to substitute other letters for the ones that are used or to supply a letter which has been omitted.

In "teaching reading" the child is taught to ignore all of these useless letters, substitutions, and omissions; in "teaching spelling" he is taught to again insert, change, or omit these same letters in the accepted or traditional order. It is as if we were to take the proverbial fly out of the ointment, then put it in again for the next person to take out and put in again, and so on to eternity.

The child must go through at least 6 years of this useless routine and then be chained to the dictionary for the remainder of his life. He learns to misspell and often to mispronounce the limited number of words required in reading simple books and writing simple stories. Where does he go for the correct pronunciation? Not to the accepted spellings but to the parentheses in the dictionary. Why make the accepted pronunciations so inaccessible? If he is to pronounce the words in the books he reads, why not write the correct pronunciations into these books? Instead, we hide them in secret compartments in a separate, unwieldy book which he cannot even handle. Thumbing through a dictionary for the correct pronunciation is a needless waste of time, not only for the child, but also for the adult. We could delegate this job to a few persons.

Norman McQuown reports, in the *Elementary School Journal* (March 1954), the construction of a perfectly phonetic written language for the Tarascan Indians of Mexico. The entire tribe became completely literate (as far as the mechanics of reading is concerned) in 3 months. He believes that all of our school children could become literate in one school term if our words were spelled as they are pronounced. Noah Webster has bequeathed to us this graphic counterpart of speech. (Perhaps we could improve on his system of diacritical marks.)

If children could learn to read and spell in one school term, we would not have to worry so much about getting more school buildings or about teacher shortages, and time would be made available for acquiring an education. We need efficiency experts in education as well as in industry.

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Martian Canals

In his article "On the rejection of the Martian canal hypothesis" [*Sci. Monthly* **85**, 23 (1957)] W. A. Webb states: "I sought, but was unable to find, an example in inanimate nature of a pattern where a preponderance of the rays issue from junctions of four or more." He presumes that the canal pattern on Mars is the result of some intelligence and, hence, that it is artificial.

I am pleased to submit an aerial photograph taken by the U.S. Air Force of an extensive, faulted, and cracked-up limestone area in the Southwest Sahara (Fig. 1). The pattern, although out of scale, is very similar to Trumpler's canal network, as illustrated by Webb. On the photograph are several mile-long intersecting straight lines, at some places (marked *x*) five, six, and even nine of them. They radiate from roundish spots (marked *z*). There are duplications and some parallelism of lines, such as that observed on Mars by Schiaparelli, but Antoniadi's "spots" are present as well. Some lines are fine, thin; others are broad trenches (marked *y*).

This terrestrial pattern is inherent in limestones of horizontal bedded plateau character. It occurs on limestones in arid regions of barrier reef origin.

The lines are fault trenches, cracks, or joints, corresponding landforms with the "canals," the round holes, sinkholes, or uvalas, similar to the "oases." The forms are accentuated by vegetation, largely in cracks and depressions where it is protected from drying winds of the semidesert. Plants find moisture in the residual red clay of the cracks. The plant hue changes with the seasons. It is vivid green in the rainy season and yellowish in the dry season. The long trenchlike lines are enlarged faults or joints, and the spots are sinkholes in the soluble limestone. Latest research by Sinton (carbon hydrogens) and by H. Strughold, Mitchell, and Koistra (life conditions for bacteria) indicates, together with the changing hues of yellowish to blue green, that plant life exists on Mars. However, to date there has been no indication of a possible, even extinct, animal life on the planet.

There are two types of features on Mars which permit the drawing of conclusions about the character of rocks covering its surface. Color photographs show differently tinted areas, ranging from near black over red-brown, yellowish brown, bluish grey, grey, and flesh color to light cream color. The second feature is visible only by sharp eye observation; it is the landforms, which in outline are usually not sharp with respect to the tinted areas and the canal-spot pattern.

The color pattern is almost identical with the color of rock outcrops on terrestrial deserts (Fig. 2). Here, basaltic rock shows blackish tone (*B*), sandstones show many different tints of red-brown and yellowish brown (*Rb*, *YBr*), shales are light grey or bluish grey (*Gr*). Limestones are reddish-cream colored; from a great height they appear flesh colored. Sands have a light cream color (*Cr*). It seems that the whole family of rocks can be found on Mars, just as on the earth. The latest tektite research [V. E. Barnes, *Geotimes* 1, No. 12, 6 (1957)] indicates that tektites, formerly supposed to be some extraterrestrial "vulcanic glass," are melted sandstones, clays, shales, metamorphic rocks, and some igneous rocks. Tektites coming from space are mainly particles of a sedimentary crust of some destroyed planet of our system.This important discovery is in accord with the "desert outcrop color pattern" of Mars.

Aerial photographs cover only a dozen square miles. The evidence is therefore somewhat out of scale. Composite mosaics, if reduced to small scale, show that the pattern is not very much related to scale. Therefore, landforms on desert areas, especially limestone plateaus, are comparable with the Martian pattern. The Hammada el Hamra, the red limestone desert in northwestern Lybia, covers hundreds of thousands of square miles. The huge basalt area in Dekkan, India, is not only comparable in shape but also in extension with the blackish "Syrtis Major" on Mars. Features similar to canals, thousands of miles long, can be found on the African Rift Valley or some hundreds of miles long on the Sahara and Australian deserts. The possible tectonic origin of the canals may be accentuated by the behavior of the Mars crust under its lower gravitational pull, which causes more pronounced fault cracks.

The striking resemblance of the canal pattern to uplifted dead limestone reef areas, its color similarity, and its place in the rock family point to the existence of limestone on Mars. On the earth, limestone reefs are built up by small animals and plants (algae). They extract the lime from warm sea water. They need floating plankton for food and

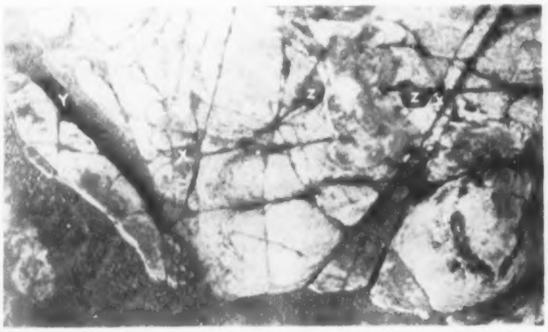

Fig. 1. Vertical aerial photograph of a limestone area in the southwest Sahara. Scale: about 1:75,000 [Courtesy U.S. Air Force]

Fig. 2. Oblique aerial photograph of differently tinted rock outcrops in Arabia. Scale: for foreground, about 1:75,000; for background, about 1:500,000. [Courtesy U.S. Air Force]

grow only if sea-level changes occur. Reefs in their living form may cover hundreds of thousands of square miles, like the Great Barrier Reef of Australia.

Conclusions to this limestone theory are tempting, although speculative: Did an ocean millions of years ago cover the Martian surface? Has the drying out process caused a constant regression of the water, leaving dead coral reefs behind? Have he reefs been built up by animals, or algae, or both? Has the evolution of organic life forms been stopped by the regressing oceans, leaving only forms of primitive structure behind?

Evidence of similarity of landforms and color points toward a natural explanation of the canals. Research, however, has not yet reached the point where the likely origin of the canals can be decided.

H. F. von Bandat
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The photograph of the southwest Sahara limestone formation which H. F. von Bandat includes with his letter is especially useful, since it gives additional data on rock crack patterns on a larger scale than I have been able to analyze heretofore. I have made a tracing of the pattern (Fig. 1) and have applied the statistical method of analysis to it. Some latitude of opinion is possible with respect to exact interpretation of the fissure lines, and others may differ, but I think that the differences will prove to be slight, and that the conclusions shown in Table 1 can be agreed upon. Since points with two rays are topologically indistinguishable from kinks in a line, such configurations were not counted. The 18 rays that run off the borders of von Bandat's photograph are not included in the count tallied under one ray per point.

Table 1. Distribution of rays among points in network patterns of nature and artifice, expressed in percentage of points in the pattern which possess the specified number of rays per point.

Rays per point	Von Bandat's Sahara limestone	Carmel, Calif., lava rocks	Ohio railroads	Trumpler's Martian canals	Lowell's Martian canals
1	23.3	0.8	1.4	8.2	3.7
2	0	0	0	0	0
3	51.2	71.8	10.9	20.5	12.5
4	19.9	23.9	47.2	42.9	54.7
5	4.1	2.3	13.7	16.3	7.0
6	0.7	0.8	9.0	8.2	5.8
7	0	0.4	5.8	3.5	5.2
8 or more	0.7	0	12.0	0.4	11.2

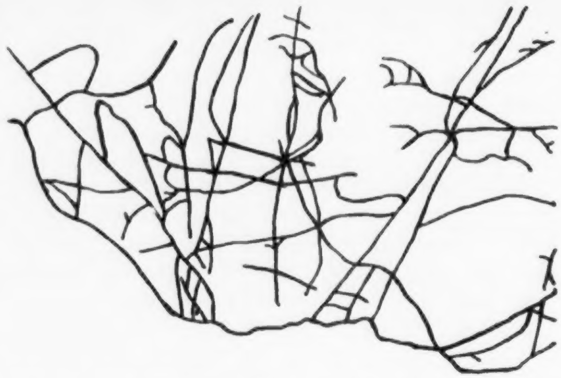

Fig. 1. Crack pattern of H. F. von Bandat's photograph of a limestone area in the southwest Sahara.

It can be seen that 23.3 percent of the points in the Sahara photograph possess but one ray. This limestone pattern is unique among those patterns of nature that I have examined in that there is such a large percentage of such points. The percentage of points with one ray is a measure of the degree by which the pattern departs from that of a true (closed) network, which would have no point with but one ray.

The Sahara limestone area exhibits a crack pattern in which a preponderance of the centers possess only three rays, and a very small proportion of the centers possess five or more rays. This is characteristic of most of the other rock patterns that I have observed. An exception, however, can be seen by anyone driving up the north bank of the Columbia River in the state of Washington near The Dalles, where the highway cuts into rock formations that are cracked on a quadrilateral pattern in which the preponderance of the centers have four rays and almost the entire remainder of the centers have but three rays. Like other rock patterns, very few of the centers have five or more rays. The same rule holds with respect to the patterned tundra areas of Alaska and other regions of ground frost.

It is evident from Table 1 and the data that I have presented in other publications, that if we accept the Lowell-Trumpler Martian network theory, we must conclude that the pattern on Mars is unlike any natural pattern that we have thus far discovered on the earth. Only communication networks such as railroads are characterized by having as high a proportion of junctions with five or more rays as the network on Mars has. Furthermore, the natural network patterns of the earth are limited to a local region or a part of a continent, whereas our communication networks enmesh continents upon the same macroscopic plan with which the canals of Mars gird that globe.

I wish to repeat the caution that I have previously expressed: we must not presume, as the result of these network analyses, that the Martian pattern has been proved to have a design requiring intelligence equal to man's for its creation. Additional evidence giving more certain criteria for patterns indicating high intelligence is required before such a view can be sustained.

If these analyses but stimulate scientists to renewed effort directed toward a clearer delineation of the canals and the pattern that they make, I would feel that the firm new knowledge gained thereby would be a far better vantage point for drawing conclusions about the existence of intelligent Martian beings than the disputed data we now rely upon.

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Instruction in Modern Hebrew

No article on "World Affairs, Languages, and Children" [*Sci. Monthly* **85**, 64 (1957)] is complete without mention of the role of modern Hebrew in the education of Jewish grammar-school children. By confining himself to public schools, the author has overlooked the fact that, for several hundred thousand Jewish parents, language instruction for their children constitutes not a novel approach but an age-old concept modified by a new insight into the learning process and by a fresh enthusiasm for which the rebirth of the state of Israel is largely responsible. Today, class instruction in Hebrew, on two or more weekday afternoons, undoubtedly benefits more young children than does the teaching of any other of the seven foreign languages enumerated by the author.

Still more ambitious are, of course, the courses offered by over 150 Hebrew day schools, located in most large and medium-sized cities of the United States, which have experienced a phenomenal growth in the last decade. Their work, strictly speaking, might not fall into the scope of the article, which disregards a similar emphasis on French, Italian, Polish, and so forth, in parochial schools of our ethnic minorities. The difference, however, of which the casual observer of the scene might not be conscious, is that these schools teach languages spoken in the homes and in the ancestral country of their students while Hebrew was termed a "dead" language until it was revived and secularized by the deliberate efforts of a handful of modern scholars.

L. SELIGSBERGER
Newton Center, Massachusetts

Controlled Genetic Development

Since I am, of course, one of the "genetically more fortunate" individuals mentioned by H. J. Muller in "Man's place in living nature" [*Sci. Monthly* **84**, 245 (1957)] (and have already begun to meet my obligation to rear a large family—12, at present) I presume to ask a few questions suggested by this article. Of course, it is still too early to expect definitive answers to these questions. But they *are* difficult questions, and, if the future evolution of man is to be carried out according to a noble plan, it is indeed time to raise them.

Granted that the evolution of man has not yet reached an end, what direction will it take if it is allowed to proceed unhampered by purposive action? Is it desirable to accelerate this evolution along its naturally determined path, or should it be directed to another road? Are there any criteria, now capable of being stated in reasonably clear terms, that can be used in defining this answer? Given that it will someday be possible to describe the genetic composition of an individual, how (in general terms) is it to be determined that a particular individual is "burdened with genetic defects" or blessed with the converse qualities?

Or should we perhaps consider a different approach in meeting the challenge that is posed? No one would deny the value of truths from the past, such as those that can be epitomized by the words *brotherly love, moral courage, wisdom*—I would also add another, dealing with the relationship between God and man—and the fact that these truths will help us to decide what qualities man should regard as being most desirable in himself and in his progeny. Certainly it is true that man has indeed an obligation—given that he will one day have the knowledge—to improve himself. But the question is, what constitutes improvement?

Does it not lie, perhaps, in an increased awareness of an individual's religious, moral, and social obligations? Is it really an improvement if we become more proficient in intellectual and social matters, in attaining to better(?) "modes of thought and living"? Would it not be wise, while we rightfully continue our investigations into the secrets of controlled genetic development, also to redouble our efforts to establish, now, instead of the sham that pervades our culture, a vital, living, cultural heritage based on the principles of religious, moral, and social responsibility? Then, when we finally know how to alter the human race, we might be able to effect a real improvement, provided, of course, that an improvement would still be deemed necessary.

JAY A. YOUNG
King's College, Wilkes-Barre, Pennsylvania

BOOK REVIEWS

The Zeppelin in the Atomic Age. The past and future of the lighter-than-air aircraft. Edwin J. Kirschner. University of Illinois Press, Urbana, 1957. xi + 80 pp. Illus. $3.50.

It is startling and refreshing in this age to find a champion of the great airships of Zeppelin. Contrary to popular belief, these ships have a long and successful history. In this country, where we have built only three and, in our own inexperience, wrecked them, we have blindly concluded that they are not, and cannot ever be, successful. Few people realize that the Germans had flown more than 35,000 passengers prior to World War I and that the list is much longer since then, with four post-World War I ships in service. The Graf Zeppelin alone carried more than 20,000 passengers. And all of this without injury to a single one of them, until the tragic and entirely avoidable fire on the Hindenburg I! The Germans built more than 120 of these great airships. But we know it cannot be done!

In general, Edwin Kirschner has, in a simple presentation, organized a vast amount of information, much of which is not generally known. The first chapter, "Historical developments," is interesting and informative reading, with the various technical terms well described. The second chapter, "Engineering characteristics," though brief, is adequate for the purpose of the nonengineer. It is written in a straightforward style, and all of the facts are correct, to the best of my knowledge.

The third chapter, "Economic consideration," is a different story. Here it is difficult to say what will be. We have the figures on the patronage, income, expenses, and profits of the past. Of the future, one cannot be certain. However, the author has relied heavily on the studies made by the Goodyear Tire and Rubber Company, the only producer of airships in the United States at this time. If anyone knows the figures, *they* should. Therefore, I believe that the author has presented a reliable and hopeful picture.

The fourth and fifth chapters, on the role of the U.S. Government and the role of international organizations, respectively, are, in general, a history of the findings of various fact-finding boards and the complete disregard of these facts by the various United States Government bureaus. When the best brains and best informed people spend months of their time, again and again, in studying this matter, and when the various groups come up with the same recommendations, it is certainly discouraging to observe the completely reversed views and actions of our leaders in Washington. The author has collected and presented his facts well, and this book should be enlightening reading for the general public. In the international area, there is not so much a record of blind prejudice as a lack of consideration. The author proposes several possible and perhaps very worth-while areas in which the airship could be a useful instrument for the promotion and protection of international peace and security.

The book is well illustrated. Its format is a bit unusual in that it is exaggerated vertically and compressed horizontally.

ANDREW J. FAIRBANKS
Rensselaer Polytechnic Institute

A Naturalist in Palestine. Victor Howells. Philosophical Library, New York, 1957. 180 pp. Illus. + plates. $6.

This is a unique book, for it concerns a country which is no longer in existence. The author visited Palestine shortly before it became divided into Israel, for the Jews, and Jordan, land of Arabs. However, the volume is concerned not with politics or international problems but with observations of the recent fauna and flora for comparison with present-day fauna and flora, which reflect the radical changes now taking place as a result of uneasy human interrelationships and altering terrain.

Accompanied by a young German naturalist, the author traveled on camel-back, camping now and then and covering the country with remarkable thoroughness, throughout an entire year. The narrative is in the form of comments on the various forms of life encountered in daily and nocturnal excursions. A great variety and a surprising number of creatures were met with, from hyenas to hawk moths and from sun birds to imperial eagles.

The interrelations among the various forms of life are a feature of the account, and many common habits, such as the formation of ant-lion pits, are described. The author has a curious habit of constantly mentioning unusual sounds. Some of these descriptions are difficult to interpret, such as the "rattling" of the "parchment" wings of bats, the dragonflies which "hurtled down in a screaming dive," or the eagle owl which swooped "with a loud noise and a rattle and swish of feathers." On

the whole, the identifications seem to be authentic, and the text is peppered with technical names.

Occasionally this naming is somewhat overdone, as where the typical pariah dogs "(*Canis putiatini*)" are technically separated into three named varieties. The tale is interspersed with vivid and interesting accounts of the customs of the Bedouins. Of especial note is the tragic story of the feast of Ramadan.

Without other comment the author remarks that "it was most unfortunate for me that all my cameras, photographic equipment and films (both exposed and unexposed) were stolen at the end of the journey." This explains the use of conventional tourist photographs, although the author has furnished a number of illustrations of his own, striking and woodcutlike in character.

On the whole, the volume makes interesting reading, and the general animal life of this strange country is vividly presented.

WILLIAM BEEBE
New York Zoological Society

In-Service Education for Teachers, Supervisors, and Administrators. Fifty-sixth yearbook of the National Society for the Study of Education. pt. 1. Nelson B. Henry, Ed. National Society for the Study of Education, 1957 (distr. by University of Chicago Press, Chicago, Ill.). 376 pp. $4.

The authors of this yearbook express the opinion that, if we are to provide the best teaching in elementary and secondary schools, it is no longer desirable to depend on the adequacy of college programs for the preparation of teachers and on the natural desire of teachers to improve through experience. As a remedy, it is suggested that all school personnel should have the opportunity to profit by participation, during the school year, in planned activities for professional improvement. Such activities are designated as constituting "in-service education."

In section I, the authors trace the history of the development of in-service education. Chapter IV presents a summary of the findings of social psychology relative to the operation of the processes of change within an institutional setting (the school), and chapter V states 12 operating principles or "guidelines" to be used in planning effective programs of in-service education. The viewpoints of teachers, of administrators, and of consultants regarding in-service programs are presented in section II, while in section III are described specific in-service programs at state, regional, and national levels. This section also includes suggestions to teacher-training institutions for the pre-service education of teachers who will later participate in in-service programs. Finally, section IV presents a discussion emphasizing the desirability of teacher interest and initiative in the launching of programs of in-service education and gives suggestions for the evaluation of the outcomes of these programs.

The authors of the yearbook are to be commended for their endorsement of the principle that actual improvement in practices associated with instruction will occur most readily as teachers work toward the solution of problems in which they feel a personal interest. It would appear, therefore, that one of the best ways to provide for improved practice would be for supervisors and administrators to seek to provide the stimulation and an environment in which alert and intelligent teachers might work out solutions to their own instructional problems. However, the entire emphasis in the yearbook is on group, rather than individual, activity. In actual practice such group, or "workshop," activities as are recommended by the authors may fail to exert appreciable influences on classroom practices because of a natural lack of homogeneity among members of the group with regard to instructional problems which have real personal significance.

The group, or "workshop," approach to the solution of instructional problems, as it is described in the yearbook, may also be vulnerable in at least two other ways, which seem not to have been sufficiently recognized by the authors. First, there appears to be inadequate recognition of the necessity for the use of authoritative sources of information (other than "consultants") during the progress of group work. Second, teachers, supervisors, or administrators who are thoroughly "sold" on the group, or "workshop," method may be inclined to lose themselves in the mechanics of the method, thus detracting from the attainment of the ultimate objective, which is the improvement of practice.

The term *action research* is frequently used by the authors, and this type of research is highly recommended as a desirable activity for in-service education. Action research is said to differ from other research in the field of education in that (i) it is conducted by "practioneers" rather than by research specialists and (ii) the "problems attacked by action research are, in the main, broad and complicated ones involving many variables which cannot be eliminated or entirely controlled. Admittedly providing less definitive tests of stated hypothesis [than other research], action research seeks its justification in claims for greater relevancy of its findings and in the greater possibility that the results of the research will be translated into

practice." Aside from the point regarding the inability to control variables, this description might fit much of the better research in education which has been published in the past. Thus, it is possible that we have here a relatively new name for a method of research which has long been recognized and accepted (although with a real attempt to control variables).

The [illegible]k varies considerably among its sections and chapters in style and degree of detail. Some chapters, like chapter IV, seem overtechnical, while others, like chapter III, include materials which should be unnecessary for many potential readers. The entire message could probably have been presented adequately by use of from one-third to one-half of the pages which have been utilized. Nevertheless, it is my opinion that this yearbook, like many earlier publications of the National Society for the Study of Education, will be accepted by professional educators as an authoritative reference book in its field.

HAROLD E. WISE
University of Nebraska

Aspects of River Pollution. Louis Klein. Academic Press, New York; Butterworths, London, 1957. xii + 621 pp. Illus. $14.50.

President Eisenhower has said that pollution of our rivers and streams is "an extravagant waste." The United States is just beginning a cleanup program that should materially reduce this needless waste. The new program, inaugurated only last year, is administered by the U.S. Public Health Service. It includes improved enforcement procedures against polluters, an expanded research program, and a $50-million grant program to help communities build sewage-treatment plants.

Already enough applications have been received from cities and towns—on a basis of seven of their dollars to one from the Federal Government—to more than use up the first year's total of $50 million. And Congress has just approved appropriations for the second year's allotment of $50 million.

Will the 10-year grant program authorized by the 1956 act do the job? Only time will surely tell. But the job is one of such massive proportions and is so complex, as well, that it is certainly one of the most challenging ever to face our country. If, as the former Surgeon General, Thomas Parran, recently said, atomic radiation is "the greatest single public health problem of the future," water pollution might be classed as number two on the list of environmental health problems—or perhaps as even coequal with atomic radiation.

Another recent news item disclosed that scientists at Fort Detrick, Maryland, the biological warfare center, have discovered that atomic irradiation can be used to eliminate sewage waste from streams and water-supply sources. They noted that studies so far indicate that waste sterilized by irradiation would not constitute a public health menace. Research began on the irradiation method while the military scientists were looking for a way to make certain that none of the deadly organisms handled in experiments at the biological warfare center escaped into nearby streams. In their experiments, according to the news accounts, samples of virus and bacterial spores were completely sterilized by isotope irradiation.

These observations are occasioned by publication of a new book on water pollution, published by our British cousins. It is called *Aspects of River Pollution* and is written by Louis Klein, with chapters by J. R. E. Jones and H. A. Hawkes.

Man has been poisoning himself on this planet ever since he crawled out of the primordial ooze. But it is only quite recently that he has had the power to poison not only himself but his habitat as well—the earth, the air, and the waters as well as future generations who will inherit them.

This is industrial man, of course. And in the process of industrializing the earth, he has had to learn to come to terms with the poisons that he himself has made. Hence, his guiding principle of research into [illegible] things today is: How much poison can I [illegible] nd still live? It is a little like asking how much whiskey can a man drink without becoming a drunkard.

We must grant that the impact industrial man makes on his environment is massive indeed. And this assumes ominous proportions when we consider that the world population will more than double by the end of the century, when today's children will be reaching middle age.

With more than 200 years of industrial experience behind them, the crowded British Isles may give us pointers. Klein's book discusses the different kinds of pollution that affect rivers, the causes, and the bad effects and reviews methods available for pollution abatement. Despite the vast amount of scientific knowledge of the past 50 years, the problem of contamination of rivers and streams is still acute.

Klein quotes the senior chemical inspector of the British Ministry of Housing and Local Government to the effect that the problem will never be solved, since population growth and new manufacturing processes will create new effluent difficulties that it will take too much time, money, and research to overcome.

That Klein does not accept this defeatist attitude is apparent from the fact that he has written this book. It is divided into 15 chapters, each of which covers a particular aspect of river pollution. He discusses the biochemical and physicochemical aspects of river pollution, detection and measurement of stream pollution, the significance and interpretation of chemical tests, and, of course, pollution abatement, with an attempt to set standards for rivers, sewage effluents, and industrial wastes. He suggests a method of determining the "standard flow" of a river, based on a statistical evaluation of a 10-year flow record. He considers this to be a more appropriate basis for calculating dilution factors than is the customarily used dry-weather flow of a stream.

Klein has included in his book references to more than 1300 original papers. He must have rejected many times this number. From the growing number of chemists, engineers, and biologists—and the increasing number of ordinary citizens—who deal with problems of pollution and their prevention, many will find this book of great assistance.

JACK DURHAM
U.S. Public Health Service

Introduction to Operations Research. C. W. Churchman, R. L. Ackoff, and E. L. Arnoff. Wiley, New York; Chapman & Hall, London, 1957. x + 645 pp. $12.

This book is a general introduction to the principles of operations research, emphasizing the applications of scientific method to operations research procedures and to the problems that operations research attempts to solve. The treatment is rigorous but deliberately simplified; and, although mature ability to think abstractly and to handle symbols is a necessity for the reader, technical proficiency in mathematics beyond the calculus is not required. The book is organized into 22 chapters grouped in ten sections, written by 15 experts; thus, the volume itself exemplifies the "team" approach to "systems analysis"—that is, the synthesis of the diverse knowledge of experts on a common methodological base.

Part I presents the general nature of operations research, the systems-analysis approach, and the "research team" type of project. Part II deals with problem-solving in general; it discusses organizational analysis and models of communications systems, how problems are logically defined, how methods are chosen in specific cases, and the evaluative weighting of project objectives.

Part III is a concise discussion of the nature of models, how models are constructed, and how they are applied to problem-solving. This serves to introduce simplified but rigorous discussions of inventory models, allocation models (including linear programming), waiting-time models (both queuing theory and sequencing models), replacement models, and competitive models. The section on competitive models includes a good introduction to game theory and a good analysis of bidding models. Following this are chapters dealing with problems of how to test a model and the solution derived from it and with the problem of controlled implementation of workable solutions. The concluding chapter is on the organizational and educational tasks encountered in establishing operations research programs.

The editors offer the following "working definition" of operations research: "the application of scientific methods, techniques, and tools to problems involving the operations of a system so as to provide those in control of the system with optimum solutions to the problems" (page 18). The goal of an operations research project is optimization of a system as a whole, but most projects start with a much smaller area, with the expectation—often fulfilled—that original conceptions of the over-all scope will change. Also emphasized is the continuing emergence of new problems, both those previously obscured and those created by the solutions to others.

The text emphasizes the application of operations research to industrial problems, and the exclusion of military problems from this text was generally a wise decision. On the other hand, while the "enormous potential application to community problems" is mentioned in the preface, the chapter on "Traffic delays at toll booths" would hardly be a happy example to offer either sociologists or social philosophers.

This excellent textbook is the product of 5 years' teaching of operations research at Case Institute of Technology. The authors are outstanding in the field of industrial operations research. The book is suitable for classroom use, for industrial study groups, and for persevering private readers. I have reservations, from both a philosophic and an industrial standpoint, about certain aspects of operations research and its theoretical foundations, and these aspects are included in the book under discussion. But those who want to understand the art of operations research—its great potentialities and probable limitations—can do no better than to start here.

SAMUEL E. GLUCK
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Lecture Notes on the Use of the Microscope. R. Barer. Thomas, Springfield, Ill., ed. 2, 1956. 76 pp. Illus. $1.50.

The author states that few changes have been made in the second edition of *Lecture Notes on the Use of the Microscope* beyond the correction of some minor errors and the clarification of certain points. Two references are added.

Barer laments the usual lack of any training in microscopy. (This training is often rather casual, also, in the United States). So he has written a very practical "do it yourself" instruction book to tell the student what to do, why it is done in this manner, and what to do when the results are unsatisfactory. Theory is included only as it is needed for the simplest answers. Likewise, he does not include phase, polarization, and other specialized types of microscopy because "many specialized modern methods are in a state of flux, and their inclusion in a book of this type might only overburden and confuse the beginner."

The first appendix, on "Some instructive experiments with the microscope," should be read, and the experiments should be performed by most of the professors and teachers in our schools and universities, especially those with a yen to write a paper on some new or aberrant form of illumination. Students of chemistry and physics as well as of biology will find here a basic introduction to the bright-field microscope.

OSCAR W. RICHARDS
American Optical Company

The Copernican Revolution. Planetary astronomy in the development of Western thought. Thomas S. Kuhn. Harvard University Press, Cambridge, Mass., 1957. xviii + 297 pp. Illus. $5.50.

Paradoxically, the intellectual stature of man increased at a time when he lost his central position in the physical universe. This paradox is not resolved by merely describing the astronomical aspects of the Copernican system or even by tracing its scientific history back through the Middle Ages to Ptolemy, Aristotle, Eudoxus, and Plato. It is necessary, first, to show how the geocentric concept became deeply involved in the cosmology and theology of the Middle Ages; second, to describe the constraint which this association imposed on creative thought; and, finally, to make clear the intellectual release which resulted when the Aristotelian spell was finally broken. Accompanying such a discourse there must be adequate astronomical detail, for, after all, the arguments must revolve around the central observational facts as do the planets around the sun. This is the difficult task, requiring a combination of scientist and historian, which Thomas Kuhn has assigned himself. In slightly less than 300 pages he has written a convincing summary which will be very helpful, not only to students and teachers, but to the general reader as well.

After a few elementary but useful facts of observation, the author describes the ancient geocentric universe consisting of two realms or spheres, one for man and one for the stars. With contributions from Plato, Aristotle, and others, this concept was the beginning of the sphere-worship which lasted for almost 2000 years and from which not even Copernicus could shake himself free. The theory of concentric spheres of Eudoxus deserves all the attention the author gives it, because it was the first important attempt to explain the complex planetary motions in terms of simpler geometric movements. It was Eudoxus who imagined that each planet moved on one of a number of connected concentric spheres whose axes of rotation were oriented in different directions and whose common center was the earth. This model led quite naturally to the Ptolemaic system, in which the planets were supposed to move on spheres (epicycles) whose centers moved, in turn, on other spheres (deferents) whose centers were the earth. Ptolemy himself added several refinements to the epicycles and deferents, and other astronomers introduced many more, so that by the 16th century the Ptolemaic system had become a monster of complexity. Nevertheless, the earth-centered universe was by then firmly integrated into the accepted cosmology, and scholars like Thomas Aquinas had reconciled Aristotle with theological dogma.

It was in this atmosphere that Copernicus published his *De Revolutionibus*, almost apologetically placing the sun in the central position formerly occupied by the earth. Today when students read, or are told by their teachers, that there was a time when people were persecuted for teaching that the earth rotated on its axis and revolved around the sun, they find this incomprehensible against the background of modern thought. For such readers this volume should be very helpful; it lives up to its title in showing why the Copernican hypothesis marked the beginning of an intellectual revolution. This is the most difficult part of the author's task, and it is well done. The remaining chapters deal with the consequences of the revolution—the contributions of Kepler, Galileo, and, finally, Newton.

It is true, as Kepler stated later, that "Copernicus failed to see the riches within his grasp." Having discarded the earth as the center of the universe, he then resorted to epicycles and other devices (except the equant, which he despised)

used by Ptolemy. The weight of tradition was too great even for Copernicus. This is unfortunate, but it is not sufficient cause for depreciating the Copernican contribution; this was the first giant step necessary before Kepler could add the refinements of elliptical orbits.

The volume contains a technical appendix and a very helpful bibliography.

HUGO N. SWENSON

Queens College, New York

Inventors and Inventions. C. D. Tuska. McGraw-Hill, New York, 1957. 174 pp. Illus. $3.75.

Patent Notes for Engineers. C. D. Tuska. McGraw-Hill, New York, ed. 7, 1957. 192 pp. Illus. $4.

Two interesting and useful monographs from the pen of the director of patent operations at Radio Corporation of America Laboratories in Princeton, New Jersey, have recently been added to the all-too-meager literature on the stimulation and protection of inventive productivity. Drawing on his long and rich professional experience in this field, which includes the acquisition of no less than 20 United States patents on his own inventions, C. D. Tuska has provided, in these two publications, many encouraging and helpful suggestions on the evolution, recognition, patenting, and marketing of inventions and discoveries.

Unfortunately, in *Inventors and Inventions*, the author's oversimplification of the case histories he cites as illustrative of methods of invention detracts from his otherwise extremely valuable contribution on this subject. The application of the same thoroughness and imagination displayed in his theoretical analysis and classification of the various approaches to invention would have greatly enhanced and strengthened this important section of the book.

In the earlier chapters, on the history of invention, the effect of environment, the influence of education, the relationship between age and scientific creativity, and the psychology of invention and discovery, the author's discussion of these subjects lacks conviction, largely because of his interpretation of, and heavy reliance on, old, previously published material. The two final chapters, on patentable inventions and on their disposition, and the five excerpts from court decisions in the appendix are so brief and sketchy that they leave much to be desired; however, adequate treatment of these subjects would probably require a full-length book in itself.

Nevertheless, despite these obvious limitations in the use of his material, Tuska has presented, in clear and easily understandable language, as well as in condensed form, an informative introductory work which deserves careful reading by scientists generally as well as by creative inventors, research directors, and patent attorneys. In view of the recognized need for arousing and clarifying thinking on the general subject of invention and its importance in our economy, it is hoped that the author will, in the near future, find time in the midst of his active work to prepare a more complete and comprehensive book, comparable in style and thoroughness to his *Patent Notes for Engineers*.

The latter publication is a "must" for everyone concerned with research, inventions, and patents. This seventh edition of a manual originally prepared 10 years ago, for use within his own company, bears the marks of the mature judgment and professional experience of the author in handling the patents of a large and productive research organization. As he says in the introduction to this edition, the book "represents a serious effort to bridge the technical gap between engineers, research workers, inventors generally, and their patent attorneys."

In my opinion, it is the best work currently available on the subject, and it offers, in sufficient detail to be of use and value both to the layman and to the initiate, information and guidance in understanding and finding one's way through the intricate maze of patent prosecution and patent protection. This book is an indispensable item in the reference library of the research scientist and the patent attorney alike. It also provides an excellent basic textbook for a short course on patent problems, which should be included in the curriculum of every school of science and technology.

ARCHIE M. PALMER

National Research Council

The Mathematics of Physics and Chemistry. Henry Margenau and George M. Murphy. Van Nostrand, Princeton, N.J., ed. 2, 1956. 604 pp. $7.95.

This well-known book was first published in 1943 and went through more than a dozen printings before its revision in 1956. The new edition contains extensive text changes and additions throughout, notably in the chapters on numerical calculations, integral equations, and group theory. A treatment of Fourier, Laplace, and related transformations has been added, and bibliographic references are listed at the end of each chapter. There are no sets of routine problems at the ends of the chapters, but worked-out examples and special problems illustrating applications of the text material are inserted every few pages.

The form is concise and to the point; clarity, rather than elegance, has been emphasized. There is a complete index occupying 18 pages, which should be of considerable assistance to those using the volume for reference rather than as a textbook. The typography is up to the usual high standard expected of the publisher.

This book is suitable as a textbook in applications of mathematics to the major theoretical problems of physics and chemistry. About half of the pages are devoted to what might be called classical topics: the partial derivatives of thermodynamics; ordinary differential equations; special functions (gamma, Bessel, and Hankel); Legendre, Hermite, and Laguerre polynomials; vector analysis; the calculus of variations; and the partial differential equations of classical physics. Later in the book there is a chapter on linear integral equations as well as a long and practical one on numerical calculations. The other half of the text is devoted to items especially related to, and useful in, quantum physics and chemistry: statistical mechanics, group theory, the mechanics of molecules, matrix algebra, and quantum mechanics.

The authors state as their aim the presentation of those parts of mathematics which form the tools of the modern worker in theoretical physics and chemistry. I believe that this aim has been successfully achieved and that the second edition of this excellent book will enjoy an enthusiastic reception, both as a textbook and for reference and self-study.

STANLEY S. BALLARD
Scripps Institution of Oceanography

Coal Science. Aspects of coal constitution. D. W. van Krevelen and J. Schuyer. Elsevier, Amsterdam, 1957 (order from Van Nostrand, Princeton, N.J.). 352 pp. Illus. $9.50.

In the preface to this book it is pointed out that there have been remarkable developments in fundamental coal research during recent years and that the group at the Dutch State Mines has made important contributions in this field. It is stated: "This book . . . though it attempts to be as objective as possible in giving a fair evaluation of the work done by others, is nevertheless subjective in the sense that the authors have placed special emphasis on the contribution known as statistical constitution analysis made by their own laboratory. It should be pointed out that the book does not pretend to give an exhaustive survey of the subjects treated." With this appraisal I am in full accord.

The contents of the book are presented in three broad parts: "Coal in its various aspects" (138 pages); "Constitution of the coal matrix" (104 pages); and "Principal physical and chemical properties" (91 pages). These parts in turn are divided into 15 chapters, covering such fields as occurrence and classification of coal, stratigraphy, petrography, coalification, ultrafine structure, statistical constitution analysis, chemical constitution analysis, electric and magnetic properties, mechanical properties, and thermal properties.

Coal classification systems are clearly discussed and summarized, and the book will be a useful reference in this field. The treatment of the composition of plant debris and the distinctions between rock types and the various macerals will be useful to workers in other fields who have been lost in the "semantic swamps" of the petrographic literature. In the discussions of ultrafine structure, the treatment of recent x-ray work and of coal porosity is adequate; that on solvent extraction is not. I cannot agree with the inference to be drawn from the authors' statement in the introductory section to the "Constitution of the coal matrix" that step-wise degradation studies for determining structure of coal are doomed to failure. As with other high molecular weight substances, complete knowledge of coal structure will probably result from the use of combinations of chemical degradations and physical tools.

Those who have followed the publications of this Dutch group will be particularly interested in the three chapters dealing with constitution analysis. The work reported in earlier publications has been digested, and some of the controversial points concerning the evaluation of the "ring constant" and relations between "fraction of aromatic carbon" and "ring condensation index" have been clarified. The sections dealing with chemical degradations are disappointing.

The few data available on electric, magnetic, mechanical, and thermal properties of coal are well discussed in three chapters. The book concludes with a chapter on the reactions of coal with molecular oxygen.

The printing and format are excellent, and an attractive feature is the use of summary sections following each of the three main parts. Few errors were noted. The formula given on page 154 for calculating the density of dry, ash-free coal is incorrect, and there is confusion in the discussion of the grindability tests on page 273.

This book can be strongly recommended and constitutes a welcome addition to the literature of coal science.

HENRY C. HOWARD
Bureau of Mines,
U.S. Department of the Interior

Books Reviewed in SCIENCE

September 6

Wing Theory, A. Robinson and J. A. Laurmann (Cambridge University Press). Reviewed by W. R. Sears.

Actions Chimiques et Biologiques des Radiations, vol. II, M. Haïssinsky, Ed. (Masson). Reviewed by G. E. Stapleton.

The Life of Bacteria, K. V. Thimann (Macmillan). Reviewed by M. P. Starr.

Changes of State, H. N. V. Temperley (Cleaver-Hume; Interscience). Reviewed by J. E. Mayer.

The Neurohypophysis, H. Heller, Ed. (Academic Press; Butterworths). Reviewed by A. B. Rothballer.

Technique of Organic Chemistry; vol. X, *Fundamentals of Chromatography*; H. G. Cassidy (Interscience). Reviewed by E. Heftmann.

Man into Space, H. Oberth (Harper). Reviewed by T. S. Gardner.

September 13

Organic Synthesis; vol. I, *Open-Chain Saturated Compounds*; vol. II, *Open-Chain Unsaturated Compounds, Alicyclic Compounds, Aromatic Compounds*, V. Migrdichian (Reinhold; Chapman and Hall). Reviewed by I. M. Hunsberger.

Battle for the Mind, W. Sargant (Doubleday). Reviewed by L. E. Hinkle, Jr.

Glossary of Geology and Related Sciences, J. V. Howell, coordinating chairman (American Geological Institute). Reviewed by E. W. Berry.

Quantum Chemistry, an Introduction, W. Kauzmann (Academic Press). Reviewed by H. S. Johnston.

Biochemical Problems of Lipids, G. Popják and E. Le Breton, Eds. (Interscience), Reviewed by J. B. Brown.

Experimental Research on Ageing, F. Verzár, Ed. (Birkhäuser). Reviewed by N. W. Shock.

A Revision of the Australian Chafers (Coleoptera: Scarabaeidae: Melolonthinae), vol. 1, E. B. Britton (British Museum of Natural History). Reviewed by O. L. Cartwright.

September 20

On Human Communication, C. Cherry (Technology Press; Wiley). Reviewed by A. L. Beeley.

Galactic Nebulae and Interstellar Matter, J. Dufay (Philosophical Library). Reviewed by O. Struve.

Analytical Microscopy, T. E. Wallis (Little, Brown). Reviewed by O. W. Richards.

A Monograph of the Immature Stages of African Timber Beetles (Cerambycidae), E. A. J. Duffy (British Museum of Natural History). Reviewed by W. H. Anderson.

The Granite Controversy, H. H. Read (Interscience). Reviewed by J. Gilluly.

The Aleut Dentition, C. F. A. Moorrees (Harvard University Press). Reviewed by G. W. Lasker.

The Principles of Heredity, L. H. Snyder and P. R. David (Heath). Reviewed by H. H. Strandskov.

September 27

Bioenergetics, A. Szent-Györgyi (Academic Press). Reviewed by B. Commoner.

Educating Gifted Children, R. F. DeHaan and R. J. Havighurst (University of Chicago Press). Reviewed by S. L. Pressey.

Plant Virus Serology, R. E. F. Matthews (Cambridge University Press). Reviewed by W. C. Boyd.

Development of Vertebrates, E. Witschi (Saunders). Reviewed by D. P. Costello.

The Genus Achlya: Morphology and Taxonomy, vol. XX, T. W. Johnson, Jr. (University of Michigan Press; Oxford University Press). Reviewed by J. R. Raper.

New Books

Colorimetric Analysis. vol. I, *Determinations of Clinical and Biochemical Significance*. Noel Allport and J. W. Keyser. Chapman & Hall, London, ed. 2, 1957. 424 pp. $9.

The Computing Laboratory in the University. Preston C. Hammer, Ed. University of Wisconsin Press, Madison, 1957. 236 pp. $6.50.

Craig and Faust's Clinical Parasitology. Ernest C. Faust and Paul F. Russel; assisted by David Richard Lincicome. Lea & Febiger, Philadelphia, ed. 6, 1957. 1078 pp. $15.

Dairy Bacteriology. Bernard W. Hammer and Frederick J. Babel. Wiley, New York; Chapman & Hall, London, ed. 4, 1957. 623 pp. $9.

Electron Microscopy. Proceedings of the Stockholm Conference, September 1956. F. S. Sjostrand and J. Rhodin, Eds. Academic Press, New York, 1957. 366 pp. $17.50.

From Sterility to Fertility. A guide to the causes and cure of childlessness. Elliot E. Philipp. Philosophical Library, New York, 1957. 120 pp. $4.75.

Glass Reinforced Plastics. Phillip Morgan, Ed. Iliffe, London; Philosophical Library, New York, ed. 2, 1957. 291 pp. $15.

The Human Brain. From primitive to modern. A. M. Lassek. Thomas, Springfield, Ill., 1957. 250 pp. $4.75.

Lens Materials in the Prevention of Eye Injuries. Arthur H. Kenney. Thomas, Springfield, Ill., 1957. 73 pp. $3.50.

Medical Radiation Biology. Friedrich Ellinger. Thomas, Springfield, Ill.; Blackwell, London; Ryerson, Toronto, 1957. 978 pp. $20.

Metallurgical Progress—3. A third series of critical reviews (reprinted from *Iron & Steel*). Iliffe, London; Philosophical Library, New York, 1957. 88 pp. $6.

Movement of the Heart and Blood in Animals. An anatomical essay. William Harvey. Translated from the original Latin by Kenneth J. Franklin and published for The Royal College of Physicians of London. Thomas, Springfield, Ill., 1957. 209 pp. $3.50.

Natürliche und Künstliche Erbänderungen. Probleme der Mutationsforschung. Hans Marquardt. Rowohlt, Hamburg, 1957. 177 pp.

Polyethylene. Theodore O. J. Kresser. Reinhold, New York; Chapman & Hall, London, 1957. 217 pp. $4.95.

Radiation Shielding. B. T. Price, C. C. Horton, K. T. Spinney. Pergamon Press, London, 1957. 359 pp. $10.

Rogers' Inorganic Pharmaceutical Chemistry. Taito

O. Soine and Charles O. Wilson. Lea & Febiger, Philadelphia, ed. 6, 1957. 705 pp. $9.50.

Swine Feeding and Nutrition. Tony J. Cunha. Interscience, New York, 1957. 296 pp. $5.

A Textbook of Dairy Chemistry. vol. I, *Theoretical*; vol. II, *Practical*. Edgar R. Ling. Philosophical Library, New York, ed. 3, 1957. 227 pp.; 140 pp. $12 per set.

Vanguard! The story of the first man-made satellite. Martin Caidin. Dutton, New York, 1957. 288 pp. $3.95.

Zinsser Bacteriology. David T. Smith, Norman F. Conant and others, Eds. Appleton Century-Crofts, New York, ed. 11 of *Textbook of Bacteriology* by Hiss and Zinsser, 1957. 966 pp.

The A-B-C of Electrons, Atoms, and Molecules. Their mechanical actions and functions and their simple ratios in mathematical terms. Frank X. Graser. Greenwich Book, New York, 1957. 104 pp. $3.

Advances in Pest Control Research. vol. I. R. L. Metcalf, Ed. Interscience, New York, 1957. 521 pp. $11.

L. P. Alford and the Evolution of Modern Industrial Management. William J. Jaffe. New York University Press, New York, 1957. 387 pp. $5.

Biology of the Treponematoses. Based on studies carried out at the International Treponematosis Laboratory Center of the Johns Hopkins University under the auspices of the World Health Organization. WHO Monograph Series No. 35. Thomas B. Turner and David H. Hollander. World Health Organization, Geneva, 1957. 278 pp. $6.

The Development and Meaning of Eddington's "Fundamental Theory." Including a compilation from Eddington's unpublished manuscripts. Noel B. Slater. Cambridge University Press, Cambridge, 1957. 311 pp. $7.50.

Fall Out. Radiation hazards from nuclear explosions. A. Pirie. MacGibbon & Kee, London, 1957. 160 pp. 12s. 6d.

Fenestella from the Permian of West Texas. Memoir 70. M. K. Elias and G. E. Condra. Geological Society of America, New York 27, 1957. 167 pp.

General Zoology. Tracy I. Storer and Robert L. Usinger. McGraw-Hill, New York, ed. 3, 1957. 670 pp. $7.50.

Handbuch der Physik. vol. XXVIII, *Spectroscopy II*. S. Flügge, Ed. Springer, Berlin, 1957. 454 pp. DM. 98.

A History of Industrial Chemistry. F. Sherwood Taylor. Abelard-Schuman, New York, 1957. 483 pp. $7.50.

Laboratory Experiments in College Physics. Cicero Henry Bernard. Ginn, Boston, ed. 2, 1957. 335 pp. $4.25.

Medical Writing. The technic and the art. Morris Fishbein. Blakiston Div., McGraw-Hill, New York, ed. 3, 1957. 272 pp. $7.

Morphological Astronomy. F. Zwicky. Springer, Berlin, 1957. 303 pp. DM. 49.60.

Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His Systems of the World. Translated into English by Andrew Motte in 1729, translations revised and supplied with an historical and explanatory appendix by Florian Cajori. University of California Press, Berkeley, 1947. 715 pp. $6.50.

The North American Deserts. Edmund C. Jaegers. Stanford University Press, Stanford, Calif., 1957. 318 pp. $5.95.

Principles of Plant Pathology. E. C. Stakman and J. George Harrar. Ronald, New York, 1957. 592 pp. $8.

Quantitative Organic Analysis. James S. Fritz and George S. Hammond. Wiley, New York; Chapman & Hall, London, 1957. 316 pp. $6.50.

The Relativistic Gas. J. L. Synge. North-Holland, Amsterdam; Interscience, New York, 1957. 119 pp. $4.50.

Rheumatoid Arthritis. A definition of the disease and a clinical description based on a numerical study of 293 patients and controls. Charles L. Short, Walter Bauer, William E. Reynolds. Harvard University Press (for Commonwealth Fund), Cambridge, Mass., 1957. 490 pp. $7.

Semiconductors. Their theory and practice. G. Goudet and C. Meuleau. Translated by G. King. MacDonald & Evans, London, 1957 (order from Essential Books, Fair Lawn, N.J.). 334 pp. $18.90.

Set Theory. Felix Hausdorff. Translated from the German ed. 3 by John R. Aumann *et al.* Chelsea, New York, 1957. 352 pp. $6.

Soil–Plant Relationships. C. A. Black. Wiley, New York; Chapman & Hall, London, 1957. 339 pp. $7.

A Student's Histology. H. S. D. Garven. Livingston, Edinburgh, 1957. 662 pp.

The Teaching of Hygiene and Public Health in Europe. A review of trends in undergraduate and postgraduate education in nineteen countries. WHO Monograph Series No. 34. F. Grundy and J. M. Mackintosh. World Health Organization, Geneva, 1957 (order from Columbia University Press, New York 27). 254 pp. $5.

Analytical Design of Linear Feedback Controls. George C. Newton, Jr., Leonard A. Gould, James F. Kaiser. Wiley, New York, 1957. 430 pp. $12.

Chemical Phase Theory. A comprehensive treatise on the deduction, the applications and the limitations of the phase rule. J. Zernike. Kluwer's, Deventer, Netherlands. 509 pp. Fl. 70.

Engineering Manpower, How to Improve Its Productivity. A special report for management by graduate students at the Graduate School of Business Administration, Harvard University, Boston, 1957. Engineering Management Reports, P.O. Box 161, Cambridge, Mass., 1957. 162 pp.

Handbook of Toxicology. vol. II, *Antibiotics*. William S. Spector, Ed.; compiled by J. N. Porter and G. C. De Mello. Saunders, Philadelphia, 1957. 276 pp.

Laboratory Guide for Elementary Plant Physiology. Rufus H. Moore. Burgess, Minneapolis, Minn., 1957. 144 pp. $3.

The Liberal Arts College. A chapter in American cultural history. George P. Schmidt. Rutgers University Press, New Brunswick, N.J., 1957. 319 pp. $6.

The Life and Work of Sigmund Freud. vol. 3, *The Last Phase*, 1919–1939. Ernst Jones. Basic Books, New York, 1957. 553 pp. $7.50.

A Modern Introduction to Philosophy. Readings from classical and contemporary sources. Paul Edwards and Arthur Pap, Eds. Free Press, Glencoe, Ill., 1957. 648 pp. $6.50.

Nuclear Chemical Engineering. Manson Benedict and Thomas H. Pigford. McGraw-Hill, New York, 1957. 608 pp. $9.50.

Pharmaceutical Calculations. Willis T. Bradley, Carroll B. Gustafson, Mitchell J. Stoklosa. Lea & Febiger, Philadelphia, ed. 3, 1957. 325 pp. $4.50.

Principles of Physical Science. Francis T. Bonner and Melba Phillips. Addison-Wesley, Reading, Mass., 1957. 752 pp. $7.50.

A Short Course in Quantitative Analysis. Hobart H. Willard, N. Howell Furman, Egbert K. Bacon. Van Nostrand, Princeton, N.J., ed. 2, 1957. 249 pp. $4.25.

Solid State Physics. Advances in Research and Application. vol. 4. Frederick Seitz and David Turnbull, Eds. Academic Press, New York, 1957. 554 pp. $12.

Steroid Homeostasis, Hypophysis and Tumorigenesis. Alexander Lipschutz. Heffer, Cambridge, England, 1957. 92 pp. 15s.

Animals Parasitic in Man. Geoffrey Lapage. Penguin Books, Baltimore 11, 1957. 320 pp. Paper, $0.95.

The Atlantic. A history of an ocean. Leonard Outhwaite. Coward-McCann, New York, 1957. 479 pp. $6.50.

Constructing an Astronomical Telescope. G. Matthewson. Philosophical Library, New York, ed. 2, 1957. 100 pp. $3.

A Dictionary of Scientific Terms. Pronunciation, derivation, and definition of terms in biology, botany, zoology, anatomy, cytology, genetics, embryology, physiology. I. F. Henderson and W. D. Henderson; ed. 6 by J. H. Kenneth. Van Nostrand, Princeton, N.J. ed. 6, 1957. 548 pp. $12.50.

The Direction of Research Establishments. Proceedings of a symposium held at the National Physical Laboratory 26–28 September 1956. National Physical Laboratory. Her Majesty's Stationery Office, London, 1957 (order from British Information Service, New York). 4 sections.

Drugs and the Mind. Robert S. de Ropp. St. Martin's, New York, 1957. 320 pp. $4.50.

Functional Neuro-Anatomy. Including an atlas of the brain stem. A. R. Buchanan. Lea & Febiger, Philadelphia, ed. 3, 1957. 362 pp. $7.50.

The Hebrew Iliad. The history of the rise of Israel under Saul and David. Written during the reign of Solomon probably by the priest Ahimaaz. Translated from the original Hebrew by Robert H. Pfeiffer; general and chapter introductions by William G. Pollard. Harper, New York, 1957. 154 pp. $2.50.

Psychopathic Personalities. Harold Palmer. Philosophical Library, New York, 1957. 179 pp. $4.75.

Scientific and Technical Translating. And other aspects of the language problem. United Nations Educational, Scientific and Cultural Organization, Paris, 1957. 282 pp. $4.20.

Soil, the Yearbook of Agriculture, 1957. U.S. Department of Agriculture, Washington, 1957 (order from Supt. of Documents, GPO, Washington 25). 797 pp. $2.25.

Systematic Organic Chemistry. Theory and applications. Hugh C. Muldoon and Martin I. Blake. McGraw-Hill, New York, 1957. 836 pp. $7.75.

Meetings

November

20–22. Structure of the Nucleus, chemical research conf., Houston, Tex. (W. O. Milligan, Rob't. A. Welch Foundation, P.O. Box 1892, Houston 1.)

22. Tritium in Tracer Applications, symp., New York. (Symp. Committee, New England Nuclear Corp., 575 Albany St., Boston 18, Mass.)

December

1–6. American Soc. of Mechanical Engineers, annual, New York, N.Y. (C. E. Davies, ASME, 29 W. 39 St., New York 18.)

1–15. Bahamas Medical Conf., 4th, Nassau, Bahamas. (B. L. Frank, 1290 Pine Ave., West, Montreal, Que., Canada.)

2–5. American Rocket Soc., annual, New York. (J. J. Harford, ARS, 500 Fifth Ave., New York 36.)

2–5. Entomological Soc. of America, annual, Memphis, Tenn. (R. H. Nelson, ESA, 1530 P St., NW, Washington 5.)

3–4. Human Factors in Systems Engineering, symp., Philadelphia, Pa. (C. Fowler, American Electronic Labs., 121 N. 7 St., Philadelphia.)

4–8. American Psychoanalytic Assoc., New York, N.Y. (J. N. McVeigh, APA, 36 W. 44 St., New York 36.)

4–10. American Acad. of Optometry, annual, Chicago, Ill. (C. C. Koch, 1506–1508 Foshay Tower, Minneapolis 2, Minn.)

5–7. Texas Acad. of Science, annual, Dallas. (G. C. Parker, Education Dept., Texas A&M College, College Station.)

6–7. Oklahoma Acad. of Science, annual, Enid. (J. T. Self, Dept. of Zoology, Univ. of Oklahoma, Norman.)

7–8. American Acad. of Dental Medicine, New York, N.Y. (S. Ross, 136 E. 36 St., New York 16.)

8–11. American Inst. of Chemical Engineers, annual, Chicago, Ill. (F. J. Van Antwerpen, AIChE, 25 W. 45 St., New York 36.)

9–11. Fluorides Symp., Cincinnati, Ohio. (Secretary, Inst. of Industrial Health, Kettering Laboratory, Eden and Bethesda Aves., Cincinnati 19.)

9–13. Eastern Joint Computer Conf., Washington, D.C. (H. H. Goode, Dept. of Electrical Engr., Univ. of Michigan, Ann Arbor.)

9–22. Southeast Asia Soil Science Conf., 1st, Manila, Philippines. (I. G. Valencia, Bureau of Soils, P.O. Box 1848, Manila.)

10–11. Water Quality Control for Subsurface Injection, 2nd annual conf., Norman, Okla. (M. L. Powers, Extension Div., Univ. of Oklahoma, Norman.)

13–14. Association for Research in Nervous and Mental Disease, 37th annual, New York, N.Y. (R. J. Masselink, 700 W. 168 St., New York 32.)

15–18. American Soc. of Agricultural Engineers, Chicago, Ill. (J. L. Butt, ASAE, St. Joseph, Mich.)

17–19. Nuclear Sizes and Density Distributions Conf., Stanford, Calif. (R. Hofstadter, Stanford Univ., Stanford.)

19–21. American Physical Soc., Stanford, Calif. (W. A. Nierenberg, Univ. of California, Berkeley 4.)

26–27. Northwest Scientific Assoc., annual, Spokane, Wash. (W. B. Merriam, Geography Dept., State College of Washington, Pullman.)

26–30. American Assoc. for the Advancement of Science, annual, Indianapolis, Ind. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5.)

The following 43 meetings are being held in conjunction with the AAAS annual meeting.

AAAS Acad. Conference, annual (Father P. H. Yancey, Spring Hill College, Mobile, Ala.). 28 Dec.

AAAS Cooperative Committee on the Teaching of Science and Mathematics (F. B. Dutton, Dept. of Chemistry, Michigan State Univ., East Lansing). 27 Dec.

Alpha Chi Sigma (R. L. Hicks, 1130 E. Jefferson St., Franklin, Ind.)

Alpha Epsilon Delta (M. L. Moore, 7 Brookside Circle, Bronxville, N.Y.). 28 Dec.

American Assoc. of Hospital Consultants (J. B. Norman, 8 South Church St., Greenville, S.C.)

American Astronomical Soc. (J. A. Hynek, Smithsonian Astrophysical Observatory, 60 Garden St., Cambridge 38, Mass.). 27–30 Dec.

American Geophysical Union (E. M. Brooks, Dept. of Geophysics, St. Louis Univ., St. Louis 8, Mo.).

American Medical Assoc. Committee on Cosmetics (Mrs. V. L. Conley, AMA, 535 N. Dearborn St., Chicago, Ill.). 28–29 Dec.

American Meteorological Soc. (K. C. Spengler, AMS, 3 Joy St., Boston, Mass.).

American Nature Study Soc., annual (R. L. Weaver, School of Natural Resources, Univ. of Michigan, Ann Arbor). 26–30 Dec.

American Physiological Soc. (F. A. Hitchcock, Dept. of Physiology, Ohio State Univ., Columbus 10).

American Psychiatric Assoc. (M. Greenblatt, Massachusetts Mental Health Center, 74 Fenwood Rd., Boston 15). 29–30 Dec.

American Soc. of Hospital Pharmacists (G. E. Archambault, Pharmacy Branch, U.S. Public Health Service, Washington 25).

American Soc. of Naturalists (B. Wallace, Biological Lab., Cold Spring Harbor, Long Island, N.Y.).

American Statistical Assoc. (V. L. Anderson, Statistical Lab., Purdue Univ., Lafayette, Ind.).

Association of American Geographers (L. L. Ray, U.S. Geological Survey, Washington 25).

Association for Computing Machinery (J. E. Robertson, Digital Computer Lab., Univ. of Illinois, Urbana).

Astronomical League (W. Garnatz, 2506 South East St., Indianapolis).

Beta Beta Beta (Mrs. F. G. Brooks, P.O. Box 336, Madison Sq. Station, New York 10). 27 Dec.

Biometric Soc., ENAR (T. A. Bancroft, Dept. of Statistics, Iowa State College, Ames).

Conference on Scientific Editorial Problems, annual (G. L. Seielstad, Applied Physics Lab., Johns Hopkins Univ., Silver Spring, Md.). 26–30 Dec.

Conference on Scientific Manpower, annual (T. J. Mills, National Science Foundation, Washington 25). 30 Dec.

Ecological Soc. of America (A. A. Lindsey, Dept. of Biological Sciences, Purdue Univ., Lafayette, Ind.). 27–29 Dec.

Metric Assoc. (J. T. Johnson, 694 West 11 St., Claremont, Calif.).

National Acad. of Economics and Political Science (D. P. Ray, Hall of Government, George Washington Univ., Washington, D.C.).

National Assoc. of Biology Teachers, annual (Miss I. Hollenbeck, Southern Oregon College of Education, Ashland). 26–31 Dec.

National Assoc. for Research in Science Teaching (G. G. Mallinson, Western Michigan College, Kalamazoo). 26–30 Dec.

National Assoc. of Science Writers (J. Troan, *Pittsburgh Press*, Pittsburgh, Pa.).

National Council of Teachers of Mathematics (P. Peak, College of Education, Indiana Univ., Bloomington). 27 Dec.

National Geographic Soc. (W. R. Gray, NGS, 16 and M Sts., NW, Washington 6). 29 Dec.

National Science Teachers Assoc. (R. W. Schulz, Emmerich Manual Training High School, 2405 Madison Ave., Indianapolis 25). 26–30 Dec.

National Speleological Soc. (Brother G. Nicholas, LaSalle College, 20 and Olney Aves., Philadelphia 41, Pa). 28 Dec.

Philosophy of Science Assoc. (C. W. Churchman, Case Inst. of Technology, Cleveland, Ohio).

Scientific Research Soc. of America, annual (D. B. Prentice, 56 Hillhouse Ave., New Haven 11, Conn.). 27 Dec.

Sigma Delta Epsilon, annual (Miss M. Chalmers, Dept. of Chemistry, Purdue Univ., Lafayette, Ind.). 26–30 Dec.

Sigma Pi Sigma (M. W. White, Pennsylvania State Univ., University Park). 27 Dec.

Society for the Advancement of Criminology (D. E. J. MacNamara, New York Inst. of Criminology, 40 E. 40 St., New York 16). 27–28 Dec.

Society for General Systems Research, annual (R. L. Meier, Mental Health Research Inst., Ann Arbor, Mich.).

Society for Industrial Microbiology, Washington Section (W. N. Ezekiel, Bureau of Mines, Washington 25).

Society for Investigative Dermatology (H. Beerman, Univ. of Pennsylvania School of Medicine, Philadelphia 3). 28–29 Dec.

Society of the Sigma Xi, annual (T. T. Holme, 56 Hillhouse Ave., New Haven 11, Conn.). 27 Dec.

Society of Systematic Zoology, annual (R. E. Blackwelder, Box 500, Victor, N.Y.). 26–31 Dec.

United Chapters of Phi Beta Kappa, annual address (C. Billman, 1811 Q St., NW, Washington, D.C.). 27 Dec.

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27. Association for Symbolic Logic, Cambridge, Mass. (J. Barlaz, Rutgers Univ., New Brunswick, N. J.)

27–28. Linguistic Soc. of America, Chicago, Ill. (A. A. Hill, Box 7790, University Station, Austin 12, Tex.)

27–30. American Finance Assoc., annual, Philadelphia, Pa. (G. E. Hassett, Jr., New York Univ., 90 Trinity Pl., New York 6.)

28–29. American Folklore Soc., annual, Chicago, Ill. (M. Leach, Box 5, Bennett Hall, Univ. of Pennsylvania, Philadelphia 4.)

28–30. American Anthropological Assoc., annual, Chicago, Ill. (W. S. Godfrey, Jr., Logan Museum, Beloit College, Beloit, Wis.)

28–30. American Economic Assoc., annual, Philadelphia, Pa. (J. W. Bell, Northwestern Univ., Evanston, Ill.)

28–30. Archaeological Inst. of America, annual, Washington, D.C. (C. Boulter, 608, Univ. of Cincinnati Library, Cincinnati 21, Ohio.)

28–30. Econometric Soc., Philadelphia, Pa. (R. Ruggles, Dept. of Economics, Yale Univ., New Haven, Conn.)

28–30. History of Science Soc., annual, New York, N.Y. (Miss M. Boas, Brandeis Univ., Waltham 54, Mass.)

APPLICATION FOR HOTEL RESERVATIONS
124th AAAS MEETING
Indianapolis, December 26-30, 1957

The list of hotels and their rates and the reservation coupon below are for your convenience in making your hotel room reservation in Indianapolis. Please send your application, *not* to any hotel directly, but to the AAAS Housing Bureau in Indianapolis and thereby avoid delay and confusion. (Exception: Members of the American Astronomical Society who wish reservations at the Marott Hotel, 2625 North Meridian Street, are asked to correspond directly with that hotel.) The experienced Housing Bureau will make assignments promptly; a confirmation will be sent you in two weeks or less.

As in any city, single-bedded rooms may become scarce; double rooms for single occupancy cost more; for a lower rate, share a twin-bedded room with a colleague. Most hotels will place comfortable rollaway beds in rooms or suites at 2.50 to 3.00 per night. Mail your application *now* to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
Rates for Rooms with Bath

All hotels have sessions in their public rooms. For a list of headquarters of each participating society and section, please see *Science*, July 19, or *The Scientific Monthly* for August.

Hotel	Single	Double Bed	Twin Bed	Suite
Antlers	$4.50–10.00	$7.00–12.00	$10.50–12.00	$14.50–19.50
Claypool	7.00–10.00	9.50–14.00	10.50–14.00	13.50–34.00
Continental	8.00–10.00	8.00–12.00	8.00–12.00	12.00–15.00
Marott	7.00–14.50	9.00–14.50	10.00–17.50	14.50 and up
Severin	6.00– 9.00	8.50–12.50	11.00–15.00	25.00
Sheraton-Lincoln	6.50–11.50	9.85–15.00	13.35–16.00	24.35 and up
Warren	6.50–10.50	8.50–12.50	12.00–13.00	25.00–35.00
Washington	5.50–10.00	7.00–11.00	11.50–16.00	18.00–45.00

------------ **THIS IS YOUR HOUSING RESERVATION COUPON** ------------

AAAS Housing Bureau
1201 Roosevelt Building
Indianapolis 4, Ind.

Date of Application ....

Please reserve the following accommodations for the 124th Meeting of the AAAS in Indianapolis, Dec. 26–30, 1957:

TYPE OF ACCOMMODATION DESIRED

Single Room .... Desired Rate .... Maximum Rate ....
Double-Bedded Room ... Desired Rate .... Maximum Rate .... Number in Party ....
Twin-Bedded Room .... Desired Rate .... Maximum Rate ....
Suite .... Desired Rate .... Maximum Rate .... Sharing this room will be:

(Attach list if this space is insufficient. The name and address of each person, including yourself, must be listed.)

....

....

First Choice Hotel .... **Second Choice Hotel** .... **Third Choice Hotel** ....

DATE OF ARRIVAL .... DEPARTURE DATE ....
(These must be indicated—add approximate hour, a.m. or p.m.)

NAME ....
(Individual requesting reservation) **(Please print or type)**

ADDRESS ....
(Street) **(City and Zone)** **(State)**

Mail this now to the Housing Bureau. **Rooms will be assigned and confirmed in order of receipt of reservation.**